THE LANCET.

VOL. II.] LONDON, SATURDAY, JULY 11, 1840. [1839-40.

LECTURES BY M. DE BLAINVILLE ON COMPARATIVE OSTEOLOGY.

THE

COMPARATIVE OSTEOGRAPHY

OF THE

SKELETON AND DENTAR SYSTEM

IN THE FIVE CLASSES OF

VERTEBRAL ANIMALS,

RECENT AND FOSSIL.

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WITH GRAPHIC DELINEATIONS BY M. WERNER;

EDITED (FROM THE FRENCH) AND

ADDITIONALLY ILLUSTRATED

WITH NUMEROUS NOTES, OBSERVATIONS,

AND DRAWINGS,

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Memoir on the Real Situation of the Aye-Aye in the Series of Mammals.*

Read to the Philomatic Society, May 16, 1816.

In my osteography of the Lemurs, I have included the Aye-Aye, of Madagascar, as one of that family, without assigning my reasons for not placing it amongst the Rodentia, as is still done by many zoologists, in imitation of M. Geo. Cuvier, who gave to the genus *Daubentonia*, as formed of this animal by M. E. Geoffroy, the name of *Cheiromys*. I have thus thought it right to publish separately the memoir, in which I think I have been able to demonstrate clearly the view I have taken of the subject; twenty-four years having now elapsed since the memoir was read to the Philomatic Society; it was afterwards presented by me to the Academy of Sciences, when I was candidate to become a member of that body. I have since made no change in the memoir, or what I may have made I have taken care to note.

A natural science, whatever it may be, is composed of two parts altogether distinct from each other—1st, The facts which do not depend on ourselves, but which are supplied to us by direct observation, and often by mere chance; 2nd, The generalisation, or the manner of combining and arranging these facts, so as to be able, with more ease, to compare and test them by analogy and induction. That part of the science to which we apply the term method, is entirely of our own creation, and quite the antipodes of the first, that is, the observation of facts. But to render our method as perfect as possible, it is essential that we should know a great number of facts, to which we propose to refer, and that, in consequence, it may be established *à posteriori*. In short, if we want a certain number of facts, and more particularly some of great importance, the *method* is necessarily incomplete, and, very often, even incorrect in some points. Now, as we are still very far from being acquainted with many points in the organisation of animals, and since scientific observation, and, above all, chance, supplies daily new facts, it necessarily follows that the science of natural history, above all others, presents a greater or less number of blanks; and thus, our method will contain a greater or less proportion of errors, which time and favourable circumstances alone will enable us to fill up and correct. These corrections are thus the essential elements towards perfecting the natural sciences, and, consequently, to correct our faults is more delightful to the scientific mind, than any disgrace which may attach to the committing an error, and particularly when that error may still be the subject of dispute. This is precisely the position in which I find myself placed with regard to M. E. Geoffroy-Saint-Hilaire. He is, at the same time, so great a lover of truth, that so far from wishing to propagate and support an opinion merely because it was his own, and aware that my opinion or mode of viewing the few authentic facts, known to both of us with regard to the Aye-

* The name of Aye-Aye was given by Sonnerat to this animal in consequence of the exclamation of the Malgoches on seeing it for the first time.

Aye (and which formed the subject of that memoir), differed from his, he has been the first to stimulate me to new researches; and more than this, he has afforded me all possible means of putting zoologists in a fit state to decide. It is this which I now endeavour to do, with all due regard to a contemporary predecessor, whose lectures I have at the same time listened to for many years.

At least seven years ago, whilst studying with my two friends, M. Constant Prévot and Dr. Russell, of Dublin, the collection of Mammals in the Museum of Natural History in Paris, after a minute inspection of their stuffed skins, when I came to examine that of the Aye-Aye, of Madagascar, it struck me that it did not belong to the order Rodentia, in which it had generally been placed by the French zoologists, and even by those zoologists who had the greatest claims to following the principles of a natural arrangement (*methode*). It, on the contrary, appeared to me to possess numerous analogies with the terminating genera of the order Maki or Lemurs. With that impression I studied successively, and with care, all the minutiæ of the organisation of this most singular animal, as far, at least, as the stuffed skin would allow; and I became so satisfied of the correctness of my first opinion, that, in the complete course of zoology which I delivered in 1811, as well as in a popular course which I gave in the same year at the Athenæum, I finally placed the Aye-Aye at the termination of the order Quadrumana; and I offered, in support of this innovation, all the reasons which I at that time had been able to collect. Nevertheless, the striking and altogether anomalous dentar system presented by the Aye-Aye, compared with that of the Makis, joined to the opinion of the French zoologists, who had always, and after a lengthened discussion on the point, placed it amongst the Rodentia,—these, I repeat, kept me in a well-grounded uncertainty, and I felt then the want of knowing some more important part of the organisation of this animal, when M. E. Geoffroy, at my urgent solicitation, had the politeness to permit me to search,—for the purpose of ascertaining if, in the remains of the only specimen in our collection, and, in fact, in Europe,—some parts of the skeleton, fitted to confirm or refute my opinion with regard to the true situation of the Aye-Aye in the Mammal series, could be found. To be brief, by the most fortunate chance, I was enabled to withdraw from the stuffed skin the entire head, forearm, and carpus, which M. E. Geoffroy placed entirely at my disposal. It is to him, therefore, that I owe the materials for this memoir, which I have the honour to submit to the judgment of the Society, and in which I propose to treat of the situation which the Aye-Aye ought to hold in the series of Mammals; and, in order to arrive at this, to prove to the Society, that when an individual limits himself to the consideration of a single organ, however important, however influential it may be on the rest of the organisation, he often runs the risk of breaking through the true *natural* analogies.

To avoid this serious error, it is necessary to have an eye to the whole organism, in at once considering all the organs, with their intimate combinations; and, consequently, the anatomy ought to be the most minute, precise, and extensive, which should guide the zoologist, whose object, *par excellence*, is the grouping animals in natural families, without attending to the greater or less facility of teaching. From the foregoing premises I have arrived at the opinion, that unless the minute anatomy of all animals, or rather of all the types of the natural genera, shall be sufficiently ascertained, we cannot arrive at a satisfactory or perfect classification.

In order to attain the object which I have in view, I shall commence by giving a detailed description of the Aye-Aye, as far, at least, as the means which I possess will enable me, uniting to that description whatever I may know of the habits of the animal, which, being always in unison with the organisation, may be deduced *à priori*.

I shall compare it, as I proceed, in every possible way with the Rodentia, amongst which the French zoologists have arranged it up to the present day, and with the Quadrumana, more especially with the Makis (Lemuridæ), amongst which I am of opinion that it must ultimately take its place; so that any one may the more readily judge of the greater or less degree of affinity which it has with either group of animals.

Lastly, I shall conclude with giving a precise and detailed account of the situation which the different zoologists have successively assigned to it, by which it will appear that I am far from being the first to draw analogies between the Aye-Aye and the Quadrumana. The greater number of foreign naturalists, those even who have least pretensions to follow a natural method of arrangement, are of the same opinion as myself; but, as I suppose, by a kind of instinct, without giving any reasons for their so doing, and, perhaps, by abandoning the rigorous consideration of the number of incisive teeth, the basis of their system, they have paid more regard to the number of the fingers and toes, and especially to the existence of an opposable great toe.

This being the case, in order to solve the question proposed, I shall show that of all the parts of an animal, the skeleton is that by which we judge best of its true affinities, the skeleton alone enabling us to trace the nervous system; that, of the different parts of the skeleton, by far the most important is the head, and especially the cranium; and that amongst all the bones which enter into the composition of the cranium, it is the sphenoid and its appendages, or rather, the series of

bones which compose the base of the cranium, viz., the basilar, posterior and anterior sphenoid, which are the most instructive. This is the case not only in the Mammals, but even in the whole of the class Vertebrata, in consequence of the intimate physiological and anatomical connection which these bones have with the more important parts of the nervous system.

Section I.—*Description of the Exterior or Stuffed Skin.*

The head of the Aye-Aye is massive and spherical, extremely large posteriorly and laterally, which, most assuredly, is not the case with the Rodentia, which have the head —considered as a whole—always elongated from before backwards, at the same time extremely compressed laterally. The Tarsiers and the Galagos, amongst the Lemuridæ, show most clearly the form of the head in the Aye-Aye. The Aye-Aye wants that curve, peculiar to the Rodentia—that projecting face, which is sometimes so striking, as in the Hare, for example, that the occipital foramen appears placed much more anteriorly than it really is.

The muzzle, properly so called, is very short, and yet sufficiently pointed, a character which is never found in the Rodentia, but exists in the Galagos and the Tarsiers; the eye of the Rodentia is placed far back.

The nostrils open altogether terminally, instead of being placed laterally, as in the Squirrels. The lower jaw being as long as the upper, as we shall see hereafter in the osteology of the head, it results, that the upper lip is not constrained to curve itself strongly backwards, in order to cover the teeth of the lower jaw, as is the case in the Rodentia, and especially in the Squirrels; the upper lip, moreover, is always entire in the Aye-Aye, and never bifid as in the Rodentia.

The eyes differ remarkably both in form and in position. In the first place, instead of being lateral as in all the Rodentia, even in the Jerboa, they are directed almost entirely forwards, as in the Tarsiers and the Galagos, and, as in those animals, they are very large, with widely-cleft eyelids; a character which indicates a nocturnal animal, an instinct observed in all the Makis.

The ears, placed as far back as in the Rodentia, are terminated by an auditory tube extremely developed, as broad as it is long, very thin and membranous, very open, and directed forwards; an arrangement which is scarcely seen in any of the Rodentia, but which, on the contrary, altogether resembles that of the Galagos and the Tarsiers.

A large and wide mouth, proportionally with the shortness of the jaws, is another character in which the Aye-Aye resembles the Lemurs, and is opposed to the Rodentia.

The face, which is, for the greater part, covered with very short hair, presents also a few tufts of long, hard, and black hair; a character found in the Rodentia, but common also to the Lemurs.

The trunk is short, massive, and not compressed on the sides, as in the Rodentia, in which it is thin and arched in its length, indicating great lightness in movement. It is entirely covered with a woolly coarse hair, which is not much tufted, and of two kinds; the first kind, long and smooth, resembles common hair; the second is woolly, which never happens in the Rodentia, which have the hair soft, silky, thick, lying uniform and smooth; this happens commonly in the Galagos, and especially in the family of the Makis.

Near the inguinal region are two very large and prominent nipples; since it would seem, as several naturalists had already remarked with Sonnerat, that this animal was suckling when it was taken. In this respect, then, the Aye-Aye resembles the Rodentia in the position of these organs, and the Makis in their number; but we must not attach to this character, drawn from the mammæ, too much importance, seeing that the Elephant and the Lamantine have only two pectoral mammæ; whilst, on the other hand, certain species of the Quadrumana have two pairs, one thoracic, and the other inguinal, as, for example, the Tarsiers.*

Finally, the tail, which, together with the teeth, appears originally to have determined the place of the Aye-Aye in the genus Sciurus or Squirrel, on the supposition of its being a true Rodent, has none of the characters of that organ, in the species of that genus. In fact, although it be equally long, it has a great number of very hard elongated hairs, more resembling bristles, and this over its entire circumference; whilst in the Squirrels, with a tufted tail, very soft hairs are found, chiefly on the lateral parts. The Tarsiers and the Galagos, on the other hand, have a long cylindrical tail, covered all over with long separate hairs, but not so hard as in the Aye-Aye.

But it is chiefly in the character of the limbs that we find the Aye-Aye approaching the Quadrumana, and removed from the Rodentia.

The pectoral extremities, as compared with the pelvic, are, it is true, very short, as in most of the Rodentia; but this peculiarity is not less remarkable in the Makis, and particularly in the lowest genera of this family. Although covered with hairs of the same kind as the rest of the body, they are terminated by very peculiar hands, forming one of the most striking characters of the animal; yet, disfigured as they are, it is easy to per-

* Since this was written, I find that there are several species of Lemurs which have three pairs of mammæ, and amongst others the Tarsiers, the Galagos, and even the dwarf Maki.

ceive that they have every possible relation with the Makis, and none with the Rodentia, not even with the Squirrels, the highest organised of this class in respect to the hands. Thus the palm, although very small, as compared with the rest of the hand, is entirely naked, as in the Quadrumana, whilst in the Squirrel the greater part is covered with hairs. The thumb, although the shortest of the fingers, as in most quadrupeds, is also the largest, which happens only in the Quadrumana, with the exception of the Seal, and here for an especial reason. It is, moreover, as free as in most Quadrumana, without, however, being more opposable; on the other hand, in all the Rodentia the thumb is either entirely wanting, at least externally, or reduced to a mere rudiment which may even want the nail.

Of the other fingers, the indicator is the most slender, and is entirely naked; a character hitherto observed in this animal alone. The fourth finger is the longest, a character which I regard as common to all the animals of the family of the Makis, but which I have never met with in any species of Rodentia. The nails are not characteristic, yet have a slight resemblance to those of some of the Simiadæ; they are broader and less terminal than in the Squirrels, in which, on the contrary, they are compressed, very much curved, thick, and sharp; in short, arranged so as to assist the animal in climbing trees, like a cat.

The proportional dimensions of these fingers are as follows:—

	Proximal Phalanx. lines.	Middle Phalanx. lines.	Distal Phalanx. lines.	Total. lines.
Thumb	8.6	6	6	20.6
Indicator ..	12.6	9	7	28.6
Median....	18	9	3	30
Ring finger	21	15.6	8	44.6
Little finger	14	9.6	7	30.6

But it is especially the pelvic extremities, which have so obvious a resemblance to those of the Quadrumana, that the consideration of this part alone has forced zoologists, even the most systematic, to arrange the Aye-Aye amongst the Makis, as we shall find a little further on. In fact, besides their proportional length, in which they resemble the long-footed Makis, they are terminated by a real hand, as complete as in any other kind of Ape. The tarsus, although narrow, is prolonged backwards by a calcaneum moderately projecting; there may be even some disproportional elongation of the calcaneum and scaphoid, as takes place in the Galagos. Finally, the sole of the foot is entirely naked, as in all the Quadrumana, which never happens in any of the Rodents. The great toe is very strong, very thick, and evidently opposable to the other toes; it is enlarged at its extremity, and has a broad, flat nail, as in the Primates. However slender the other toes may be, the fourth is a little longer than the rest, even than the median, as in all the Makis; but, besides, the second toe presents a character which is peculiar to the Makis, being shorter than the others in its osseous portion; on the contrary, it is proportionally longer in the nail, and it is evidently more slender, straight, and less arched than the others. It is to be admitted, however, that this character is not so well marked as in the other Makis.

I give here, as in the hand, the measurement of each phalanx, made with the greatest care.

	Proximal Phalanx. lines.	Middle Phalanx. lines.	Distal Phalanx. lines.	Total. lines.
1 Great toe	6.6	0	6	12.6
2 Toe.....	10	5	5.6	20.6
3 Median..	12	8	5.6	25.6
4 Toe.....	11.6	8.9	5.9	26
5 Little toe	10.9	7	5.6	23.3

From the above we see that the fourth toe is a little longer than the middle one; and the second, with a nail proportionally longer, is shorter than the fifth, since it is to the rest of the toe as 5½ to 15½, whilst for the middle toe, for example, it is only as 5½ to 20.

From this detailed and comparative description of the external forms of an individual female of the Aye-Aye, it appears to me, already, that there exists no character classing it with the family of the Rodentia in general, and of the Squirrels in particular; whilst, on the contrary, the anomalous position of the mammæ is the only opposing character to its being arranged with the Makis, in which family I accordingly propose placing it. The examination of those parts of the skeleton at our disposal leads, as we shall find, to the same result.

COURSE OF LECTURES

ON THE

PHYSIOLOGY AND PATHOLOGY OF THE BLOOD,

AND

THE OTHER ANIMAL FLUIDS,

DELIVERED IN THE SESSION OF 1839-40,

AT THE

School of Anatomy adjoining St. George's Hospital.

BY HENRY ANCELL, ESQ., SURGEON.

LECTURE XVIII.

Blood and nerves.—No "nervous system" in a physiological sense.—Functions of the nervous masses and cords.—Mental operations; sensation; voluntary motion.—Sir C. Bell's respiratory system.—Dr. Marshall Hall's excito-motory system; reflected and instinctive actions.—The ganglionic system; involuntary actions; emotions and passions.

—Functions referred to nerves without sufficient proof.—Microscopical structure of the brain and nerves.—Essential dependence of nerves; nervous action, and nervous power upon the blood.—Effects produced upon the blood through the nerves: direct; general; secondary; partial.—Principles of medicine.

GENTLEMEN:—Many of the experiments detailed in the last lecture illustrate, in part, the effects of vitiated blood upon the functions of animal life. These functions are referred by physiologists to a power or powers with which the nervous masses are endowed, and of which the nerves are believed to be the conductors. I have now to call your attention to the effects produced upon the blood through the nerves, and it will be necessary to recapitulate shortly what is best known respecting the "nervous system" in the wider acceptation of that term.

It has been again and again stated in this theatre by Dr. Wilson, that "physiologically speaking there is no such thing as a nervous *system*." That is to say, there is no regular dependence or co-operation of all those functions which are clearly and unequivocally referrible to the cerebrum, cerebellum, medulla oblongata, spinal marrow, ganglions, plexuses and nerves. The olfactory is almost as completely distinct from the excito-motory function of Dr. Marshall Hall, as the optic and auditory are distinct from voluntary motion, general sensation, thought, and the passions, or these from each other. There is no known means of operating primarily and simultaneously upon these various functions of animal life, or in a manner so as to show an essential dependence the one upon the other, or the whole upon each part, as must necessarily occur if their unity were a reality. Unlike the sanguiferous system, there is no real anastomosis between nerves, but every, the minutest fibril, ministering even to the same sense, or to the functions of the same part, proceeds uninterruptedly from its origin to its terminus, whether that be single, a net-work, or a connecting loop. Unlike the blood-vessels, which uniformly contain blood, conveyed to every part of the body, nerves included, for at least one general purpose, no part of which blood can be modified without affecting the whole organism, the nerves are, to a great extent, isolated in structure and function. They can be reached universally, in a secondary manner only, through the blood. The use of the expression "nervous system" may be convenient, but we must no longer be led astray by a false idea. There is no consent between, no general combination of those phenomena of animal existence which belong to the anatomical system of nerves, excepting through the blood, and accordingly there is no physiological nervous system.

To the complicated apparatus now in question, belong many of the highest and the most incomprehensible of the phenomena of living beings. The most learned, the most talented, and the most industrious, have engaged themselves of late years in investigating the uses of its different parts, and in the discovery of the laws by which its operations are governed. Their success, although partial, is progressively leading to useful results; and the only circumstance to be regretted is, that enthusiasm, guided by the exclusive medical doctrines of the day, has bestowed upon it a theoretical importance, far beyond that warranted by the utmost extent of our knowledge. We were long taught to believe that the operations of life, without exception, were totally under the controul and dominion of the nervous system, little or no importance being attached to the blood. The nervous power has been identified with, and substituted for, that vitality which is common to every living particle, whether of nerve or blood, of animal or vegetable. At variance with facts, and absurd in itself, the "nervous system" has been made infinitely paramount, in all our reasonings, to that living stream with which it is throughout so carefully and abundantly provided, and upon which it is so completely dependent. But let us revert briefly to its functions.

Intellectual Faculties.—Although we know not the nature of the connection between mind and matter, there is little reason to doubt that the brain is the seat of all the mental operations. It is the organ through which the mind influences and is influenced by the body. The common centre of perception and consciousness, and of attention, memory, imagination, judgment, and volition. Innumerable observations concur to show that different parts of the complicated organisation of the brain perform different functions; but, notwithstanding the elaborate investigations of Gall and Spurzheim, so little is known upon the subject, that we cannot at present designate, with any degree of certainty, the distinct offices of cerebrum and cerebellum. The most important circumstance at present determined is, I believe, the great probability that the grey substance of the nervous masses is the source of power—the seat of the higher functions performed by this part of the animal organism, and that the medullary matter contains the organs by which the various nervous influences are transmitted to and from the different parts of the body.

Sensation, with all its modes of pleasure and pain, belongs equally to animal life. It is the result of impressions communicated to the sensorium by the nerves, but not by the nerves as a system. By the olfactory nerve, for smell; the optic, for sight; the portio mollis, for hearing; and for taste and general sensibility, by those fasciculi, or those fibrils of the trigeminus and of the thirty pairs of spinal nerves,

which, whether distinct or bound together with nerves possessing other functions, are traceable to the posterior tracts of the spinal column. They are, in common, the conductors of impressions from external objects, and frequently from internal operations to the nervous centres. Although it is not very easy to trace the second pair, and can be done, perhaps in imagination only, with respect to the first, we are probably not wrong in the belief, that they are all derivable from the grey substance of the spinal cord; but except in this respect, they are totally distinct from each other; and the observation applies not only to those belonging to different senses, and to the distinct fasciculi of all the various nerves of the economy, but there is reason to believe that it applies to every, the minutest fibril, of which any of the nerves are composed.

Voluntary Motion, the result of a mental act of volition, is another function performed through nerves. Through the motores oculorum, the pathetici, and the abducentes, for the muscles of the eye; the portio dura, for the face; the motor branches of the trigeminus, for mastication; the hypoglossal, for the tongue; and the nerves proceeding from the anterior branches of the spinal cord, for general voluntary motion. They are derivable, like the nerves of sense, from the grey substance of the spinal column. Through them the will acts upon the contractility of the muscular tissue, and produces all those movements which form so important a part in the history of our animal existence.

The Respiratory Movements are a series of actions performed by the higher classes of animals, for the purpose of bringing the blood into direct communication with the air, so as to ensure its arterialisation and the continuance of life. According to the views of Sir C. Bell, the three divisions of the eighth pair, the pathetici, the portio dura, the phrenic, and the external respiratory, contribute to these complicated actions. They include nerves both of sensation and volition; but it is a disputed point, and it is difficult to determine, how far the will is concerned in their ordinary operation. By some physiologists it is maintained, that the nerves of this system resemble in structure and development the voluntary nerves, and are distributed universally over the body as the organs of emotion, passion, and instinct, and that every sympathetic connection between parts, is performed by them in a reflex manner, through a portion of the spinal marrow. The respiratory nerves being mainly derived from the respiratory tract, the direct effect of an injury of the medulla oblongata upon their functions, and the circumstance that these functions are mostly performed for one specific purpose, the aeration of the blood, gives them as good a title to be called a system, in a physiological sense, as any other series of nerves and nervous actions. Yet any one of them may be divided, and its influence destroyed, without affecting the others; and in a majority of instances they are only partially associated together, in the various operations to which they belong.

Reflected and Instinctive Actions.—A series of actions, common to all classes of animals, and dependent on the nervous masses and cords. By most physiologists they are called instinctive and sympathetic; but these actions have been increased in number, associated with others, and classed as a distinct *excito-motory* system by Dr. Marshall Hall; they include phenomena in which, impressions being made in various parts of the body, motions are excited. Dr. Hall believes these phenomena to be totally independent of sensation, volition, and accordingly, of the brain, and that they depend upon a distinct power, possessed by a distinct system of excito-motory nerves, the centre being a distinct part of the spinal cord, which is the combiner and dispenser of the influence, and which may be called the true spinal marrow. To this system belongs respiration, and the respiratory system of Sir Charles Bell. All the complex motions of deglutition, and of the expulsion of fæces and urine, in fact all those of ingestion and egestion, and the actions which guard the orifices and outlets; suction, erection, ejaculation, the action of the uterus. To it is ascribed all those motions which take place during sleep and coma, and those which can be excited in the limbs and other parts of a decapitated animal. It gives tone to the muscles, and upon it depend all the complex motions in health and disease which have been called sympathetic, including the appetites, and the effects of the emotions and passions. This function is never performed spontaneously, and it never sleeps; it includes a stimulus carried along an incident nerve, to the medulla oblongata or spinalis, and reflected thence along the motor nerves. The nervous trunks in which the excito-motory system is comprised, are the par vagum pre-eminently, the respiratory nerves of Sir C. Bell, and the regular nerves of sensation and volition before enumerated, and all the actions included in it are dependent on the integrity of the nervous circle through the true spinal marrow.

Professor Müller, who has investigated the reflected and instinctive actions very minutely, explains them in a different manner. He denies the existence of any distinct system, and regards the phenomena as the result of impressions conveyed by sensitive fibres to the spinal marrow and brain; or, in the absence of the latter, to the spinal marrow alone, and thence reflected along motor nerves. There is no anatomical distinction of nerves for the functions in question, the performance of many of which are attended with true sensation; and although

the brain, as the organ of consciousness, is necessary for sensation, yet the spinal marrow is possessed of sensibility to impressions—it is capable of receiving and re-acting upon the very same impressions which, propagated to the brain, produce sensation. In Dr. Hall's system the passions are seated near the medulla oblongata; they are made a part of the excito-motory phenomena. But the passions take place only in consequence of perception. As modes of thought they have the same seat as the intellectual faculties above designated, and there is no evidence that their corporeal effects are produced through channels different from those of the other mental operations. In this instance the distinction between the functions of the brain and those of the true spinal marrow fails. Again, when we are in the act of reading aloud, the mind may be completely abstracted, yet the words in the book before us are seen, the impressions are conveyed to the brain, and motions belonging unequivocally to the voluntary system are performed, since the words continue to be pronounced. In this, and many similar instances, the mind being, as it were, cut off, it is not inferred, that seeing and articulating are performed by a set of nerves distinct from those which are sentient and voluntary, although we have a stimulus conveyed along incident nerves to the medulla oblongata, and reflected hence along motor nerves. Comparing the reflected and instinctive motions with the corporeal manifestations of mind, there is no clear line of demarcation either between the structure or the actions; and with respect to the *power*, certainly in the case of sensation and volition, for instance, there is a power superadded, viz., mental power: but this brings us back to the universally-admitted principle, that *mind*, to which we refer certain phenomena, is distinct from *matter*, to which we refer others. The law of reflex action, so much dwelt upon, is by no means confined to the phenomena included in this system, for those belonging unequivocally to the sentient and voluntary nerves are generally, if not always, reflex actions; the difference being, as we have seen, that in the latter, mental operations intervene. Withdraw the mind, or its material organ, and reflex actions take place in consequence of the same impressions, conveyed through the same apparatus, now limited in extent; and they are frequently more direct, more energetic, more marked, for the want of the controlling power. It can even scarcely be said, that spontaneous or voluntary motions are direct, and not reflex, considering the presumed seat of volition. Prevost and Dumas long since remarked, and Fletcher repeats the observation, that the nervous influence does not necessarily traverse the brain; "it may pass directly from the termination of the sensiferous to the commencement of the motiferous nerves."

Whatever explanation may be adopted by the profession of these reflected and instinctive actions, which, for the most part, are clearly functions of various nerves and nervous structures, we are deeply indebted to Dr. Marshall Hall for his laborious investigations on the subject, and for numerous facts and improvements in this, as well as in so many other parts of medical science and practice.

Involuntary Motions.—As the motions of the heart, and the peristaltic action of the intestines. These are distinguished from the former in being independent of the brain and spinal marrow. They have been associated with every vital and natural action; and in order to establish the "nervous system" of the moderns, the whole have been made dependent upon a distinct set of nerves. For this purpose the ganglionic system has been, in the first place, exonerated from all participation in the actions and relations of animal life; and, in the second place, has been invested with a supreme controul over the organic functions. Dr. Fletcher, one of the most strenuous advocates of this doctrine, maintains that the ganglionic nerves are the immediate seat of irritability, fairly stating that he uses the term *irritability* as the synonyme of *vitality*. Many other physiologists have meant, but have not declared the same thing. In order to support the doctrine that the organic functions are dependent on the ganglionic nerves, it is assumed that these nerves are universally distributed; that their action continues uninterruptedly when they are isolated from the cerebro-spinal system; that no impediment is offered to the function of a nerve of this system by its division; and that "every point of every nerve, containing both white and grey matter, may be considered as a centre of nervous energy to itself." All this is necessary to reconcile the facts, that irritation of any particular part or ganglion, e. g., the cardiac ganglion, is not propagated elsewhere; that destruction, or removal of a ganglion, does not entail an immediate diminution of vital power in the organs to which its nerves extend; that "the heart of a frog continues to beat with its ordinary rhythm, even when the entire base of the organ—when the auricles, as far as their juncture with the ventricles, are cut away," and many other analogous facts. If we admit that the vitality of every organised tissue, that all its vital operations are dependent upon it, they must be "seated in" it exclusively. In this case we should separate the ganglionic system from the nervous system, as originally understood, since the laws of its actions and phenomena are totally different. The term ganglionic power must be substituted for vitality; and we may declare boldly that we have located, if not dis-

covered, the living principle. Those who advocate this doctrine make the ganglionic nervous matter to be seated in the parenchyma of the tissues, and they either deny or overlook the vitality of the blood. The latter can be clearly demonstrated, the former is purely hypothetical.

The corporeal effects of the emotions and passions are unquestionably produced through the brain and nerves; and since they are always the result of corporeal or mental pleasure or pain, their actions are frequently, if not always, reflex. Sir C. Bell refers them to his respiratory system. Dr. Hall to the excito-motory system, and the true spinal marrow. Many other physiologists to the ganglionic or sympathetic nerves. Seeing that the last-mentioned nerves are abundantly distributed to the heart and abdominal viscera, and that their branches accompany the blood-vessels, probably throughout the body; calling to mind the innumerable phenomena which led the ancients to make the heart the seat of the passions, and the effects of the emotions and passions upon the involuntary organs, there is strong reason for believing that these nerves are the channels through which mental and other nervous impressions produce their effects upon the heart, the capillary vessels, and the processes of nutrition and secretion. It is extremely probable that the ganglionic nerves have some influence over the rhythmic operations of the involuntary organs, and that by them an intimate union is established between the animal and organic operations. Admitting this union and unquestionable influence, which explains many phenomena of life in the higher orders of animals, we arrive at the limit of those functions which are well established as due to the "nervous system."

The action of the heart and the peristaltic motion of the intestines, were attributed by Glisson, Haller, and Soemmering, to an inherent principle of irritability; but the discovery of Scarpa, that the cardiac nerves are distributed to the muscular substance of the heart, seems to have sanctioned the opinion, since entertained, that nervous power is its cause. This has been a subject of the warmest discussion; but the latest experiments are in favour of the Hallerian doctrine. Some of those, for instance, detailed in the last lecture (p. 440), by which it appears that substances received into the blood act upon the irritability of the heart, and upon that alone. Dr. J. Reid has shown that the exhausted irritability of a muscle is recovered just as speedily after the division of its nerves as when these remain intact; and narcotics will destroy the functions of the nerves, leaving the muscular irritability entire. It was stated, on a former occasion, that the attempt to prove that the respiratory actions between blood and air are the result of nervous influence, has entirely failed. Of animal heat—the sudden rush of warmth to the face; the general elevation of temperature; the rapid changes which result from the exciting or depressing passions; the increased heat during the action of a muscle; in fact, all rapid and momentary changes, whether general or local, observed in states of excitement and depression, are explained by the effects produced on the actions of the heart and respiration, by the modified circulation, and molecular changes in the blood itself, without making this a function of the nervous system. Müller remarks, that the experiments of physiologists have failed, completely, to establish the dependence of the capillary circulation on the influence of the nerves, although it is certain that nervous agency may cause an accumulation of blood in parts, and we observe sudden changes in the condition of wounds after violent affections of the mind. With respect to nutrition, it is clear that the nerves, as well as every other organ, are produced by one and the same power. "There is no fact to show that the sympathetic nerve has a more especial influence on nutrition than the cerebro-spinal nerves, except, perhaps, that the nutrition of a part does not cease when the nerves which it receives from the brain and spinal marrow are divided." That the secretions may be influenced by nervous impressions on the heart, and the circulation of the blood, there is no doubt. There are many facts in relation to the kidneys which depend upon this principle; but there are others which unquestionably depend upon alterations in the circulation, and the qualities of the blood, from other causes. Finding that the dependence of secretion upon the brain and spinal marrow is not tenable, the ganglionic system has been fallen back upon; but an interesting fact is quoted by Müller. The mammæ, which are so pre-eminently secreting glands, are not supplied with nerves directly from the sympathetic, but from the third and fourth intercostals. Thus it appears, that neither secretion, nutrition, animal heat, the action of the capillaries, nor the action of the heart, has been proved to depend upon the nervous system; and yet the assumption of this dependence has been acted upon for many years in the practice of physic.

To the anatomical system of nerves, then, belong the corporeal effects of all the operations of the mind, sensation, voluntary motion, the appetites, emotions and passions, all our instinctive actions, and the influences which these functions, strictly animal, exert over the vital and natural, or, in other words, the organic processes. In them consists our animal, as far as it can be so regarded, in contradistinction to our organic life. It has ever to be kept in mind, that the actions

and powers which it comprehends, are totally dependent upon the vitality and nutrition of the special structures to which they belong, and upon a uniform, ample, and continuous supply of arterial blood.

Thus much with respect to nervous function; but in studying the relations which subsist between blood and nerve, we must consider some points respecting the structure of the latter. The brain was regarded by the ancients as a kind of gland, in which animal spirits were elaborated, and the nervous trunks were looked upon as channels, for the purpose of conveying these spirits to all parts of the system. I think it can be collected that Mr. Hunter believed his materia vitæ diffusa, of which he considered the brain chiefly composed, to emanate from the blood. Gall and Spurzheim have more recently taught that the white nervous filaments always have their origin in the grey; they observe, that whenever the former pass through, or come in contact with grey matter, they receive additional filaments, and their bundles increase in size; hence they denominate the grey substance the "matrix" of the nerves. These opinions have been since tested by microscopical observations on the structure of the brain and nerves.

It was established by the delicate injections of Ruysch, and the microscopical observations of Leuwenhöck, that the cortical matter of the brain consists of "an incomprehensible number of minute vessels." Ehrenberg describes the cortical blood-vessels as becoming invisible in a thick vascular net-work, and surrounded by a substance consisting partly of minute grains, with a coarser grained matter, disseminated; he entertains no doubt that the grains proceed from the blood, and that some relation subsists between these grains, and the commencement of the nervous fibrils "for the distillation or preparation of the nervous substance:" the cortical substance also contains a few very fine serpentine fibres, which are most distinct, and interspersed with larger and less numerous blood-vessels in the neighbourhood of the medullary substance. The medullary substance of the brain is composed of a set of varicose, jointed, or articulated tubes, lying like parallel fibres, having an internal cavity, and containing a peculiar "nervous fluid." They are direct and enlarging continuations of the cortical fibres above described; they converge to the parts at the base of the brain, where the nerves arise, many pass into the spinal marrow, and thence proceed to the spinal nerves; and all doubt of their arrangement and structure is removed, by their direct transmission into the cylindrical tubes as follows:—The nerves consist of a set of straight, uniform, cylindrical hollow tubes, containing a "viscid, white, medullary matter;" they are generally larger than the former; they are found chiefly in the nervous trunks, and the tubes previously described pass into the latter by gradually losing their bead-like enlargements. The cylindrical tubes composing the nerves are formed into fasciculi, and these again into larger bundles; each individual bundle, and the entire cords, but not the individual tubes, being surrounded with a vascular covering, a continuation of the pia mater, and they never anastomose. The grey substance of the spinal marrow and other parts, exactly corresponds with the cortical, and the white portions with the medullary substance of the brain, in its microscopical structure; the optic, auditory, and olfactory nerves, are immediate continuations of the unchanged varicose tubes of the brain. Like the brain, the retina contains colourless globules, "excreted from the vascular structure, as grains immediately emancipated from the blood;" and in the grey nervous matter are the points in the whole economy in which depositions from the blood corpuscles take place. There is a seeming identity of structure in the sentient and motor nerves; but, according to Remak, the sympathetic nerve contains peculiar "organic" fibres, originating from the ganglions; they are pellucid, as if gelatinous; not tubular, but naked; and they accompany all the cerebro-spinal tubuli, just as these tubuli accompany all the organic fibres: the ganglions contain granules, or globules, which he describes as nucleated, and the nervous fibres always arise from their very substance.

I do not put these microscopical observations forward as being so well established, that they ought to determine our opinions respecting the structure and the functions of the nerves; but as to the tubular formation, the tendency to become varicose in certain parts, the existence of a viscous medulla, more fluid in the brain than in the nerves, the absence of anastomoses, and the peculiar granular matter diffused through the vascular structure of the grey matter of the ganglia as well as of the brain—I say, with Dr. Holland, that we must recognise the truth of the observations by the general accordance of the results obtained by Treviranus, Müller, Valentin, Remak, and Burdach; and with respect to the inferences drawn, they are at least as plausible, and less contradictory among themselves, than many of the opinions founded upon physiological experiments, which have in recent times prevailed.

In whatever way we may work out the problem of the animal functions, the modes in which they are produced, and the influences which they again produce upon the functions more immediately vital, one thing is certain, they are all alike dependent upon the circulating blood. Turn to Table VI., and we find that blood contains cerebrine,—a peculiar matter, the most complex of animal principles, besides the usual organic ele-

ments—composed of sulphur and phosphorus. This cerebrine is furnished in great abundance by the chemical analysis of nervous matter; and as the fibrin deposited from blood to form muscle, or phosphate of lime in bone, are related to the functions of bone or muscle, so this peculiar matter must have some relation, although less obvious, to the functions of the nerves. Recur to Lecture V. and the physiological history of the blood corpuscles. There is a direct relation between the nervous powers of animals and this remarkable part of the vital fluid, the blood corpuscles. The arrest of the free flow of blood into a nerve, interrupts both the power over the muscles and sensation; look, therefore, to the special anatomy of the circulation for the supply of blood to the nervous structures. The most effectual provision is made to prevent any mechanical interruption in its flow to and from the brain and spinal cord. The most complete provision is also made, in every point of the nervous structures, to secure, not only an ample supply, but a free circulation of the vital current through them. In the cranium we observe the tortuosity of the arterial canals, and the sinuses with their parietes so bound down as to prevent their collapse at any time; by which contrivances, an ample and equable stream of blood, through the cerebral masses, under all ordinary circumstances, is secured. A somewhat analogous mechanism is found in the structure and position of the large veins of the spinal cord. We have seen that every fasciculus, of every nerve, is surrounded by a fine vascular net-work. Add to these facts the observations of Soemmering and Keil, and those above quoted, in which all are agreed that the grey substance of the nervous centres and ganglia are composed of minute congeries of vessels, and that everywhere the nervous terminations are closely surrounded and in relation with them. Then direct the mind's eye to the periphery. Observe the ample supply of blood to the nerves in this part of their course; the vascularity of the skin, the organ of general sensibility; the papillæ, composed principally of blood-vessels, inclosing nervous twigs, those for the sense of touch; those for the sense of taste; the vascularity of the Schneiderian membrane; the remarkable vascular expansions connected with the retina. Take a fact made out by Magendie, that the secretion from the vessels of the pia mater, of a fluid from the blood which everywhere lubricates the nervous masses and cords, is absolutely essential to the performance of their functions. Lastly, bear in mind the facts daily and hourly before our eyes, in which nervous actions are modified, by innumerable circumstances influencing the circulation, and the molecular changes in the blood; the effects of food, wine, exercise, heat; the effects of a few particles of alcohol received into the blood; of medicines and poisons, as evinced by many of the experiments detailed in the last lecture. The influences of the qualities, quantity, distribution, and motion of the blood, over thought, and in sleep and stupor, dreaming and delirium. The whole of these, and many other considerations, must convince us that there is no nervous structure without blood, —no nervous function without blood—that nerve, nervous action, and nervous power are wholly and at all times dependent on the Blood.

The proper subject of the present lecture is, the effects produced upon the blood by nervous influences, but like many other subjects connected with the blood, little is known respecting it. Assertions, or false facts and conjectures, might be collected in abundance. It has been assumed, whenever an animal dies from the effects of a shock, from lightning, or a blow on the stomach, for instance, that death is the result of an impression on the nervous system. In cases of sudden death of this kind, it generally happens that the only post-mortem unnatural appearance, is a remarkable fluidity of the blood. The blood, as explained on a former occasion, has been deprived suddenly of its vitality. There is no proof whatever, that the effect is produced through the nervous system, it is much more probable, I should say much more philosophical, to regard it as a shock to the vitality of the blood. By such a shock, the molecular vital actions of the blood, and accordingly of the whole living system, are instantly brought to a standstill. Not that I overlook the fact, that sudden nervous impressions may have similar universal results; but where these occur, I believe it is always, in a secondary manner, through the blood. Mr. Thackrah quotes a remark made by Mr. Bell, his assistant, that in taking blood from dogs, when they show alarm, and are under the influence of terror, coagulation takes place almost immediately; but when the fright subsides, this change does not occur for one or two minutes. Many cases are on record, where persons have been immediately struck as if with apoplexy, and expired on the spot from joyful tidings, and other emotions; but I scarcely need remark, the great probability that, in most of these cases, some abnormal condition of the heart or circulating organs existed. The colour of the blood seems to be affected by nervous influences; the passions occasion sudden changes in the colour of the skin. Slight changes in the colour of the skin, also, occur frequently, and rapidly, in acute diseases, which are referred to nervous influences upon the blood, but are most probably produced mediately. There is an interesting statement made by Mr. Vines (Lancet, vol. xi. p. 423), that if the spinal marrow of a horse or ass be divided as close to the brain as possible,

the *moment* respiration ceases, the arterial blood becomes as dark coloured as venous, and as low in temperature.

Yet it is doubtful whether any direct general effects are ever produced by nervous influences upon the blood. At all events, we appear to be in possession of very few facts relating to the subject. Not so as regards secondary effects, and those which are more partial in their immediate operation. Without making the involuntary motions, and the organic functions dependent upon a nervous system, we have seen that they are influenced, especially in the higher orders of animals, by nervous impressions. M. Poiseuille found, by direct experiment with the hæmadynamometer, that moral impressions act upon the heart, and modify the force with which the blood moves. In a dog, the mercury suddenly rose at the moment when the instinct of copulation was awakened. We have no difficulty, therefore, in comprehending, nor surprise, that mental emotions will alter the force and distribution of the blood, and produce all the results of an increased quantity circulating in a part. Thus the cutaneous arteries of the face and neck are suffused with shame, and the vessels of the genital organs are distended by the sexual desire. Thus also mental causes will act on the natural secretions. The passions will produce a flow of tears; the ideas of taste a profuse flow of saliva; parental affection even in brutes a secretion of milk. Dr. Parry recites the case of a lady who had long ceased to nurse, but in whose breasts a copious secretion of milk was produced by hearing any child cry. Thus, also, anger will flush the face, and produce epistaxis or apoplexy. Courage and confidence will sustain the action of the heart, and prevent syncope under fatigue and hæmorrhage. Surprise will bring on vomiting of bile, or diarrhœa; and in the same way, by altering the force and the distribution of the blood, mental emotions will produce or renew inflammatory affections, such as gout and rheumatism. On the contrary, the author last quoted, states, that he has seen a patient labouring under incipient fever, with extreme heat of the skin of the head and neck, a very quick and full pulse, and a violent headach, nearly cured in a few seconds by the operation of fear; and the ordinary effects of apprehension, fear, dread, over the circulation, and distribution of the blood, the diminution of blood circulating in the extremities, and on the surface, and the effects of many of these emotions increasing the secretion of urine, are very well known.

But it is not by sudden impulses of emotion and passion alone, that the force with which the blood moves, its molecular actions, its distribution, the secretions which it pours out, and accordingly its composition and qualities, are changed. This power of the mental faculties over the heart, the arteries, and the blood, illustrated in the fact that a single thought may alter every healthy action of the body, produces its effects also upon the scope, and rapidity of the respiratory motions, and thus interferes with the vital changes between blood and air. Mental pleasures of the more serene kind, contentment, and hope, equalise the motion of the blood, and maintain its perfect formation; pleasurable ideas and passions of a more lively kind exhilarate the blood, augment its vitality, and the infinite and inscrutable vital actions which constitute it a living fluid. Mirth and joy increase the quickness of the pulse, the action of the heart, the fulness and rapidity of breathing, the heat of the body, the secretions and excretions, the lustre of the eye; and the muscles being rapidly supplied with a more vital fluid, there is a general increase of vigour and activity. Every circumstance tends to show that the healthy molecular actions in the blood are increased under these influences; and hence it is that they become powerful remedial agents, especially in chronic complaints. Mental distress and pain, as sorrow, melancholy, anguish, despair, have a contrary effect upon the blood; laborious and slow respiration, sense of oppression and anxiety, the phenomena of sobbing and sighing, all indicate that the vital changes in the lungs are interrupted; the heart and large vessels become distended with blood of imperfect formation, and this is not duly circulated; hence torpor in every living part, paleness of countenance, coldness of extremities, contraction and sinking of the general surface, want of appetite, deficiency of muscular force, and sense of languor; hence, the importance of these influences also, in etiology and therapeutics.

Nor have the intellectual faculties a less decided power over the living blood; not only is the faculty of attention the first to be disturbed in its operations by modifications of the blood circulating in the brain, and the first to recover itself after a feverish delirium, as the quantity and quality of that blood regains its healthy standard; but, as remarked by Dr. Holland, this faculty, directed towards the heart, accelerates or disturbs its beats; directed towards the respiration, appears first to retard and then to quicken it. Rivetted on some object of thought, the faculties of the mind evidently modify the circulation of blood in the brain; and attention directed habitually to various organs, may produce disease in them, the effects depending in great measure upon the vascularity of the parts, but by no means being limited to the organs of voluntary influence: similar remarks may be made respecting the operation of other mental faculties on the blood. These effects, and many of those before hinted at, are evidently im-

pressions conveyed through nerves; they can scarcely be referred to nerves of sensation, since we must then admit two actions in opposite directions along the same nerve, they do not belong to voluntary nerves: hence it is, that these intellectual as well as emotional influences upon the organic functions, are referred to the ganglionic cords.

We wonder at the complexity and intricacy of the anatomical system of nerves; but when we consider our intellectual and moral life, our desires, aversions, affections, passions, and instincts, our faculties of sensation and volition, our life of relation with all the objects which surround us, and with the vital organism within us, our wonder is transferred from the comparative simplicity of the instruments, to their incomprehensible effects. But giving due weight to all this, we see no reason to believe that the vital organism is essentially dependent, although necessarily connected* with, the powers and instruments of nervous action; nor do we find any justification of those principles of medicine, in which it is taught, that the material causes of diseases, and that medicinal agents, produce their effects primarily and essentially upon the sentient extremities of the nerves, that these effects are propagated by the "nervous system," and that the notion of fundamental modifications of the blood is a mere chimera.

LECTURES

ON THE

ORGANS OF REPRODUCTION IN THE ANIMAL KINGDOM.

DELIVERED IN THE ROYAL COLLEGE OF SURGEONS, LONDON.

By Professor Owen, F.R.S.

LECTURE X.—May 12, 1840.

In concluding the consideration of the class Insecta of the Articulate division of the animal kingdom, in the preceding lecture, it was shown that insect life is divided into two principal periods, during which certain portions of the visceral system predominate. Thus, in the larval condition, the digestive apparatus is at the maximum, while the generative system is simple and rudimentary; and in the imago, or perfect condition, the generative portion has assumed an imposing importance over the digestive. In the division of the animal kingdom at present before our notice, the sub-regnum Mollusca, both systems are combined in a proportion relative to their wants, in the same individual, through all the periods of life.

Tunicata.—Many of the tunicated genera are fixed immoveably to the same spot during their entire existence, and many are aggregated together so as to constitute a compound group of distinct animals. This compound form not being the result of gemmation, as in the compound polypes, but arising from the peculiarity that the ova contain many yolks and many embryos. Such is the nature of the polyembryonic ova of some of the Ascidiæ, which remind us of duplex monsters, from the circumstance of a connection subsisting between their bodies at some one point of their surface. Some of these Ascidiæ form a rounded group of living beings, having the appearance of being composed of an aggregation of stellæ disposed in a particular order. The centre of the star in these creatures is formed by the junction of all the cloacal openings of a number of individuals, while the peripheral points are the mouths of the animals. The ova are extruded from the parent in a clustered mass; and the embryos, after their development, have the power of increasing their numbers by a process of gemmation, producing, by means of their high organisation, all the essential parts which characterise the parent. Another genus of compound Ascidiæ resembles in its form a ramified coralline, a number of individuals communicating by means of a common stem, in which the general circulation is performed; but in this case each Ascidia possesses a distinct pharyngeal cavity and alimentary canal, a distinct respiratory organ, a simple sacculated liver, and an ovarium. In the Botryllus there is a double ovarium and oviduct. In a higher group of Tunicata, the individuals are larger and single, they are either sessile or pedunculate. Their mode of generation is similar to that of the compound animals, but the ova contain only a single yolk. The Cynthia has a simple granular ovarium. In the Bothelia the ovary is bilobed, and provided with two ducts.

Development of the Ova.—The vitellus, as in the progressive stages of other ova, becomes divided upon its surface into a multitude of small granules, in which the changes operating the formation of the embryo take place. On bursting through the chorion the embryo presents a form very similar to the tadpole, and is furnished with cilia over every part of its periphery, with the exception of its greater end, by which it afterwards fixes to the surface of some neighbouring body. On becoming fixed the body of the embryo shortens, and two orifices are produced; the one being the oral, the other the anal opening. In many

* Carpenter's Principles of Physiology.

of the fixed Tunicata there is no apparent difference between the male and female generative organs; but when their contents are examined with the microscope, in some, spermatozoa alone will be observed, in others ova. These essential parts have never been found combined in the same individual; it follows, therefore, that they are distinctly diœcious.

The next family of Molluscous animals is furnished with a more compact external covering than the mere membranous tunic of the preceding class. It is hardened and consolidated by the deposition of calcareous earths, and divided into two segments or valves. The animals composing this family are grouped into two classes, Pallio-branchiata or brachiopoda, and Lamelli-branchiata. In the Pallio-branchiata, the lobes of the mantle are simply vascular, and papillated upon the internal surface, without any development of branchial lamellæ; and they have two elongated and spiral organs situated near the mouth. In the Terebratula the ovarium is placed in the most protected part of the animal, immediately beneath its shell; but, towards the breeding season, it increases in size in the direction of the lobes of the mantle; and, finally, almost entirely conceals them from view. These animals have never been examined alive, and with the aid of the microscope; it is, therefore, impossible to say whether distinct sexes exist on different individuals, but their analogy with higher Mollusca would lead to that supposition. Although so extremely abundant in a fossil state in our geological collections, sometimes alone forming whole strata, yet, at the present day, they are extremely rare, and are scarcely found in the seas of our latitudes. The engravings, observed the lecturer, which I have been enabled to give of the organisation of these creatures, were made from preserved specimens obtained by dredging the sea at enormous depths, and brought to this country by Mr. Cuming. From the circumstance of the Brachiopoda being all attached to a fixed point, by means of a peduncle, it has been argued that coitus is impossible, and that, therefore, their generative organisation must be hermaphrodite. Such reasoning as this cannot be admitted at the present day, when we have brought before us the instance of diœcious plants, and among them the palms, possessed of distinct sexes, yet often separated to a considerable distance. In this case we see the winds and the insect tribe engaged in the office of conveying the fertilising pollen to the stigmata of the females.

With regard to these marine Mollusca, we know that cilia are abundantly distributed over almost every part of their surface, by means of which currents of sea-water are conveyed into the shell, bearing on their streams the fertilising atoms of the male secretion, which are thus brought into direct contact with the ova. In this manner, distance and impossibility of coitus can be no impediment to propagation.

In the common bivalve Lamelli-branchiata the ovary and testis are situated in the base of the foot of the animal. When the foot is wanting, as in the Oyster, the organs of generation occupy an analogous position. In one genus only, viz., in the Cyclas, a distinct ovary and testis have been seen occupying a place at each side of the animal. Siebold and Wagner have recently been engaged in establishing the presence of distinct generative organs in the different sexes of the fresh-water Muscle (Anodonta), though their distinct existence was pointed out and described upwards of one hundred and fifty years since by Leuwenhöek; in some he found ova, in others minute spermatic animalcules. The rediscovery of these spermatozoa in the male fluid of all animals is of comparatively recent date. There is a considerable difference of form between the male and female bivalves; so much so that conchologists, guided by external figure, have separated them from each other under the designation of distinct genera.

In the Anodonta, the ova are formed in a simple cæcal ovarium, from whence they pass along two canals by a circuitous route into the cavity of the external branchiæ, and some few into the internal branchiæ, where they undergo their development. De Blainville described the passage of the ova from the ovarium, with their escape from the shell, with so great an appearance of truth, that when Jacobson found small living beings moving about with great activity between the branchial lamellæ of the Anodonta, he was induced to consider them as parasitic animals, and gave them distinct names. These were, however, the young of the Anodonta.

Development.—In the progress of the ova from the ovarium, the germinal vesicle disappears, the albuminous matter of the chorion is greatly increased, and the vitellus breaks up into small masses, which assume a regular pentagonal form. In the next place, the yolk begins to revolve upon its own axis, increasing gradually in rapidity, and then to perform elliptical gyrations within the circumambient albumen. In about four or five weeks from this time, in May and June in the Unio, and in September in the Anodonta, two valves are discerned having dentated processes on that part of the shell which is opposite the hinge. At this period they are moving with great activity within the chorion, surrounded by the streams of fresh sea-water brought within the shell by the branchial cilia of the parent; and in this manner, by endosmosis through

the chorion, the fluid in contact with embryo is efficiently aerated.

The form of the ovum in the Molluscous tribes is very various; in the Unio and Anodonta it is spherical, in the Anomia elliptical, and in the Mytilus pointed. In the ovum of the Anodonta there is more fluid between the chorion and the vitellus than in other genera. In the embryo during its development, the internal surface of the mantle is marked by projecting papillæ; a condition which is permanent in the pallio-branchiata: but in the lamelli-branchiata, these papillæ become developed, during the progress of growth, into lips and lamelliform branchiæ. The foot at this period is long and filamentous, and probably serves the double office of an organ of touch and an organ of attachment.

The higher or Encephalous Mollusca, those possessed of a supra-œsophageal or cerebral ganglion, present many instances of an hermaphrodite organisation with complicated and singular combinations, in which no progressive gradation appears to obtain. Among the lowest instances of this group, are those which are provided with wing-like locomotive organs, the Pteropoda, as the Clio borealis, the common food of the whale; the Hyalæa, the Cliodora, &c. In these may be observed a glandular organ, described by Cuvier as an ovary, but which is actually a testis, a vas deferens which passes by the side of the oviduct; two large accessory cæcal glands, and a distinct and large-sized penis which was overlooked by Cuvier, but has been since pointed out by Eschricht. Cuvier has mistaken in this instance the ovary for the testis, and *vice versâ*; but the examination of their contents by the aid of a microscope, distinctly proves the true nature of each. The pulmonary and terrestrial Gasteropoda, as the common snail, are hermaphrodite in their generative organisation, while the marine genera are diœcious. The slug presents us with the simplest condition of combined male and female organs. The testis is partially surrounded by the liver; the vas deferens joins the oviduct, and proceeds with it to the generative opening on the right side of the neck, the usual position among Gasteropods; connected with these ducts near their termination, is an accessory sacculus, the common generative sac, which terminates at the common opening. In the Helix pomatia is an ovarium with a long oviduct, which is complicated by numerous small cæcal appendages named the multifid vesicles, and representing the prostate gland. The male portion consists of a well-formed testis, a long vas deferens which accompanies the oviduct, and has communicating with it the prostatic sacculus of Cuvier, into which opens a long cæcal follicle terminated by a dilated appendix. In the common garden snail this cæcal follicle is large, and a very important organ, and communicates with the vas deferens. Near the generative opening is a muscular cavity, the preputial sheath, which contains a small calcareous spiculum sharpened very acutely at one extremity: this organ is used as a means of stimulation previously to the coitus; and with it the two concurrent animals transfix each other repeatedly, as an excitement to the copulative act; hence it has been named the spiculum amoris: it is not unfrequently broken in these exercises, and is then reproduced.

In the diœcious tribes there is a distinct testis near the caudal portion of the animal, a prostatic sac complicated by a cæcal pouch, and a long and spiral vas deferens, which passes along the middle of a large and remarkable intromittent organ to its extremity. In the female, there is a large ovary occupying the same position with the testis in the male, a plicated oviduct complicated with the multifid vesicles, and a large and muscular vulval cavity.

The ova in terrestrial and pulmonary Gasteropods are deposited, singly, in the earth, and are hatched by the warmth of the sun. In the marine genera they are expelled in assembled masses, and surrounded by a nidamental secretion produced by a distinct organ. In those Gasteropods in which the young are developed in the terminal part of the oviduct, as the Paludina vivipara, there is no necessity for accessory glands and nidamental organs, and the generative organs are consequently extremely simple.

In the Patella and in the Chitons the generative organs are simple, and composed of little more than the essential parts. Though fixed permanently to the same spot, they possess distinct sexes on separate individuals.

RESEARCHES
INTO THE
PATHOLOGY, PHYSICAL SIGNS,
AND
TREATMENT OF PERICARDITIS;
ILLUSTRATED BY CASES.

By THOMAS MOORE, *Secretary to the Dublin Medico-Chirurgical Society for* 1840-41.

[Read before the Dublin Medico-Chirurgical Society, during the Session 1839-0.]

DOUBTLESS, Mr. President and Gentlemen, some apology must be offered to the members of this Society for again bringing before their notice a subject so often introduced, so frequently investigated, and one formerly the source of so much controversy. With feelings of the greatest reluctance and diffidence, it is, that a treatise on this disease has been composed for this evening's discussion; but as his plea, the author offers the

rarity of some of the cases it is his intention to subject to their observation.

Taking for granted that the history of the disease, he now speaks of, has been accurately traced, its symptoms minutely described, and its pathology entered into at full length, by those who have preceded him, it is not his wish at present, to occupy the Society's time by reiterating what has been already said; but proceed to give an account of the cases themselves, collected within the last few months, with the kind permission of Drs. Graves and Stokes, in the wards of the Meath Hospital.

CASE I.—*Acute Metastatic Rheumatic Arthritis—Inflammation of Pleura and Pericardium, with Effusion—Displacement of Left Lung above the Clavicle—Gradual subsidence of same during the Absorption of the Fluids—Tartar Emetic—Calomel and Opium—Recovery.*

April 19, 1838. Nicholas Kennedy, a coppersmith, 28 years of age, of robust frame and healthy constitution, was attacked, fourteen days since, after wet feet and exposure to damp, with shiverings, headach, and other symptoms which usually usher in an attack of fever. The following day he was annoyed by slight shooting pains in the hips and knees, removing in the course of a few hours from the latter to the ankles, and from thence to the small of the back. During the four ensuing days he remained suffering considerably, without applying for the means of alleviating them; the pains continuing in regular succession to shift from joint to joint, and at length becoming so severe, as to disable him from remaining at work. Ten days ago he was admitted, labouring under these maladies, in a low typhoid condition, unable to move in bed from one side to the other, occasioned by the agonising pains produced in the various afflicted parts; his countenance was collapsed; the powers of life depressed to a great degree; the knees, ankles, and shoulders, much swollen, and affected by the slightest pressure. He complained of great thirst, total loss of rest, and a dislike for every species of food. The heart and anterior part of the chest, on examination, afforded the usual phenomena of healthy viscera, with this exception, that the first sound of this organ did not retain its natural strength, whilst the second was particularly well defined.

To the parts most pained leeches were applied; and nauseating doses of tartar emetic administered; the former reducing the swelling of the joints, and allaying his extreme sufferings; the latter, instead of accomplishing its intended effect, producing profuse sweats. For the four following days he remained in a state of tolerable quiescence, sometimes allowing himself to be raised in bed, without experiencing the same uneasiness, when, suddenly, the wrists and fingers became stiff, swollen, and exceedingly pained by the least degree of pressure; the parts before mentioned, especially the shoulders, being proportionally eased, though not completely relieved. Venesection to fourteen or sixteen ounces, and the further application of leeches were then employed, attended with the same results. As soon, however, as the pains in these joints were mitigated, he was next troubled with slight vagrant pains about the chest, confined principally to the left side, and occasional beatings of the heart, of a very severe character; the tartar emetic was still persevered in, and this part of the thorax being examined by one of our most intelligent and zealous pupils, Mr. Rogers, a distinct *frottement* was discovered, but whether situated in the pericardium or pleura, he could not for certain assert.

He lies at present on the back, evidently labouring under great distress, as manifested by his looks and general aspect; his countenance is anxious and sunken; the breathing quick, short, and distressed, forty in the minute; the slightest motion in bed causes an indescribable sense of suffocation; he describes his pains to be of that distracting character, as to resemble those produced by the rack; the articulation of a few words occasions so much uneasiness, that he shudders at speaking; and the attempt at inspiring deeply, is quite impossible. Tongue white and loaded; pulse 108, regular, and of good strength.

"Percussion over the præcordial region, and antero-superior portion of left side, sounds completely dull; respiration is feebly audible in the inferior parts, but heard over all portions of the side, especially above. Above the clavicle there is a distinct puffy tumefaction, contrasting with the opposite side, and possessed of a doughy or rather elastic feel. On applying the stethoscope, the respiratory murmur with a slight sonorous râle, is distinctly audible; the jugular veins at this side of the neck are quite turgid, and occasionally an undulatory motion is observed in them. Percussion over postero-inferior portion as high up as sixth rib, is also dull; respiration intensely bronchial, and voice œgophonic. The heart's impulse is very feeble, the first sound being much impaired in strength, the second remarkably clear and distinct, are attended not with the ordinary *frottement*, but an occasional 'clicking noise;' abdomen soft, and bowels free. Let him be cupped over the region of the heart, and from ten to thirteen ounces of blood removed, and take two grains of calomel with a quarter of a grain of opium every three hours.

20. "Percussion over the superior part of left side improved, the dulness being decreased; the supra-clavicular tumefaction is less evident, the respiratory murmur is still audible in what remains; the same

elastic feel is communicated to the touch, when the remaining displaced portion of lung is pressed on; percussion over the inferior part of this side is quite tympanitic, from the stomach being distended with flatus; the bronchial respiration without crepitating *râle*, and the œgophonic voice is not so well marked; impulse of heart more perceptible; sounds, especially the first, more easily distinguishable from each other; the successive 'clicks,' or 'clicking sound,' is no longer audible, but between the mamma and sternum there is a rough uneven sound, not exactly *frottement*, but somewhat of its nature." About twelve ounces of blood were taken, and eight pills swallowed. He finds considerable ease in his breathing, and with regard to the pains generally, his features are more composed, and spirits less dejected; sweated profusely, but slept none; the gums are in a slight degree spongy; breath fœtid, and copperish taste in his mouth; pulse 108, regular, and of same character; resp. 28, more tranquil. Continue the pills.

21. The improvement in the sound on percussion, and of the fixedness of this side of the chest, the parietes acting with more freedom, is progressive, but still the resonance of the affected side remains comparatively dull; all trace of the tumour above the clavicle has disappeared, and the respiratory murmur is increased in strength superiorly and inferiorly, the bronchial respiration being considerably diminished, and mixed with sonorous, cooing, and other bronchitic râles. The œgophony, in like manner, has become indistinct; no alteration could be detected either in the impulse or sounds of the heart, the second, as usual, predominating; nor could the roughness noted on the 20th be any longer heard; pulse 84, full, and compressible; resp. 24, tranquil. He can move the joints of the lower extremities without inconvenience; the pains elsewhere also less acute; countenance more lively. Repeat the pills, and let him have a pint of beef-tea daily.

22. The supero-anterior portion of the affected side is now equally clear with the right breast when percussed; in this part, as far as the mamma, the respiration is still feeble, but distinct; posteriorly, percussion remaining much duller; the respiration above the angle of scapula is very loud, but not bronchial; below this it partakes of the bronchial character, but in a minor degree; heart's action and sounds much the same; on sitting up, the impulse becomes stronger, and there does not exist the same disparity between the two sounds; gums are slightly tender, breath very fœtid, but as yet no ptyalism.

After the 23rd, he kept his ground, daily progressing towards convalescence, without the supervention of any untoward symptom; the sounds of the heart being watched, the impulse and first sound were reported on 24th to be more vigorous, no rubbing sound having been discovered. On the 27th he was quite free from pain, all signs of effusion having disappeared, the heart's impulse, action, and sounds being perfectly natural, no abnormal sound having occurred during the intervening days.

From the history, symptoms, and physical signs, detected by mediate and immediate auscultation, can we for one moment entertain a doubt, as to the possibility of inflammation of the pericardium and pleura having existed here? Surely not; if any reliance can be placed in our methods of investigating thoracic diseases; or could we have a better marked case of inflammation of the serous membrane of the heart with effusion, more accurately corresponding to various descriptions given, from the days of Corvisart down to the present? In the first place, we have in the history, the immediate attack of the pulmonic viscera, preceded by a disease affecting nearly all the joints, their fibrous structures in particular, therefore the more to be dreaded —that erratic form of rheumatism, generally so disastrous, so much calculated to excite our alarms, to arouse our attention, and create in us the most unfavourable prognosis; corresponding to that form of disease which Dr. Latham lays such stress on in his lectures, as one means of guiding us in the discovery of pericarditis. Here is the man seized with pains and swellings in the ankles and wrists to-day, of the most violent description; before night, they shift to the knees and shoulders, exciting as much distress in these parts as in their original seat; leeches are applied, the inflammatory swellings are subdued, but in a very short time they pounce on some healthy part, or returning to their primitive situation, affect it in the same manner, and to the same degree, being again removed either spontaneously, or by some local means, and thus alternating from joint to joint, they ultimately fix on some vital portion of the body, thereby bringing into the utmost danger the patient's life, and producing those signs hereafter to be noticed.

Forewarned as we were by the history, we were daily awaiting some announcement of the organs of circulation and respiration being involved by the disease then prevalent in the system; nor were we disappointed, inasmuch as when active depleting measures were employed, the pains ran from one joint to the other, and at length were transferred to the thoracic viscera. So far we were materially assisted, in having the patient directly under our eye, almost from the beginning; but moreover, the symptoms were illustrated here almost to perfection, viz., the pungent burning pain, succeeded by palpitations, characterising the onset; the quick, hurried

breathing, the indescribable anxiety about the looks, the sensation as if on the point of being suffocated, and the other complaints recorded, all tend to confirm the certainty of the existence of pericarditis; besides the history and symptoms, we did carefully examine into the physical signs whereby the presence of an enormous quantity of fluid was ascertained, seated in the pleural cavity, the accompanying phenomena, as well those of the voice as of the respiration, ratifying the supposition.

It cannot be denied, that although every circumstance which has been brought forward, would induce us, almost without further inquiry, to pronounce acute inflammation of the pericardium to be present; yet there is not one symptom of that long catalogue, without exception, that could not be, and has been, produced by pleuritis, whether effected by the metastatis of rheumatism, gout, or other diseases affecting the fibrous structures of the joints, or by acute primary inflammation of this serous membrane, including the "friction sound," heard by my friend Mr. Rogers one or two days previously. What symptom has been mentioned, what symptom is absent, so peculiar to this disease, which cannot be declared as frequently attendant on inflammation of the pleura as of the pericardium?

Thus, then, the affair, considered in another point of view, assumes quite a different aspect, and instead of there being no doubt as to the certainty of pericarditis, we are plunged into the very depth of uncertainty, it being quite a guess on our part, whether there was an inflamed state of the heart's envelope or not; and in no respect are we wiser, more capable, or more authorised in affirming for certain its existence, than those who were searching out a means and method of discovering this complaint, in the days of Corvisart, and prior to his time. Perhaps, indeed, it would have been passed by unheeded and uncared for, under the supposition that the thoracic distress proceeded from the accumulation of fluid in the pleural cavity alone, had there not been manifested a strange, curious, and striking phenomenon, situated above the clavicle of the left or unengaged side, the evident fulness, or tumefaction of the lower portion of the neck, augmented by coughing, or rendered more prominent, elastic to the touch, and affording the characters before described. What brought this here? What could be its nature, or what morbid lesions had taken place to produce it? These questions naturally suggested themselves to the bystanders, and those who examined the parts with the greatest accuracy and minuteness. The report which has just been read, and the characters of the tumour will fully convince us, that it could be nothing else but the upper lobe of the left lung. The vesicular murmur, the râle, the clearness on percussion, and the elasticity of the feel, would not allow of our hesitating from at once pronouncing it a part of the lung, displaced. This ascertained, it remained for us to discover the cause, and the mode of accounting for it.

You remember it has been stated, there was not the slightest difficulty in determining on a pleuritic effusion; moreover, it was found that this did not occupy more than half the pleural cavity. Was the displacement of the lung occasioned by this? Decidedly not by it alone. Many here have seen cases of pleuritis, with very copious effusions, where the whole side, from the clavicle down to the most remote part of that side, has been dull, before and behind, when percussed; where the respiratory murmur has been so feeble, as scarcely to be heard, or changed into that of the most intense bronchial character, and yet no such phenomenon was conspicuous. Two cases of this kind, I had an opportunity of witnessing within the last few weeks, at the Royal Infirmary, in which percussion over the left side, from the clavicle downwards, was so dull, that you might as well strike a piece of marble, for the purpose of eliciting sound. In the one was loud bronchial respiration over all parts, with an œgophonic state of the voice about the spine of the scapula; whilst in the other, not the remotest trace of the vesicular murmur could be detected. In both persons the heart was dislocated far to the right side; yet in neither case could such a phenomenon be discovered; and certainly in the example before us, it could not be effected by the quantity of fluid contained in the chest.

Trusting that it has been satisfactorily proved, that the knowledge of pericarditis, participating in the production of the agonies this poor fellow suffered from, was obscure prior to taking into account this tumour; so now it must be equally evident, that an inflamed pericardium acted a very prominent part throughout, and must have exercised a material influence over those circumstances which tended to bring to view so interesting a physical sign, to the best of my knowledge unnoticed, and undescribed.

No explanation has yet been offered: before doing so, it is right that the friction sound heard on the preceding day be recalled to mind. See what a change a few hours brings to pass; mark with what rapidity the effusions are poured out; the patient's life despaired of; the most intense sufferings created; the pericardium and pleura becoming filled with liquid at the same time; the functions of the heart and lung being interfered with; each hastens to terminate the patient's existence. Between these two compressing fluids, accumulating with astonishing rapidity, the lung is not allowed time to accommodate itself to the diminished capacity of the place intended for it by na-

ture; downwards it cannot go, the fluid in the pleura is pressing from below; press towards the right side it cannot; the mediastinum, already displaced and occupied by the pericardium, becoming every moment more distended, prevents it; and thus finding it impossible to remain in its accustomed position, from one fluid pressing laterally and from before, backwards; the other from below and behind, upwards and forwards; it is necessarily obliged to seek out an egress, and is forcibly expelled by those two opposing and irresistible forces, to pass beneath the clavicle, and abide in the lower part of the neck.

Imperfectly described as this is, such would appear, from reasoning, to be the cause of this "supra-clavicular tumour;" corroborating in the fullest these opinions, by following out the termination of the case, we shall find the results to be of the most satisfactory description. Do we not find, after our antiphlogistic measures were enforced, an alleviated state of all his pangs? the sense of immediate suffocation removed; the accelerated and distressed breathing lessened. And are we not to attribute this generally improved condition to a check put to the secretion in the pericardium and pleura on the one hand, and a partial absorption of it on the other? This being undeniable, we should expect greater space in the pleural cavity, and a consequent decrease in the new phenomenon, if our conjectures as to displacement of the lung were correct. How do these statements stand with regard to the next day's notes, are they confirmed or contradicted—is the tumour merely one of imagination? The best and fairest umpire we can select will be the following note:—"Percussion over the superior portion of the left side is improved; the dulness decreased; the supra-clavicular tumefaction is less distinct, the respiratory murmur being still audible in what remains; in lower part, respiration is not so intensely bronchial; the voice also is less œgophonic, &c. &c."

Is it requisite to proceed further, to enlarge more fully, and to reiterate the progress of the symptoms and signs?—methinks not; sufficient having been said to show how essentially we were assisted in our diagnosis, and how beautifully ended this curious phenomenon.

There are many other circumstances in this case I would fain treat of, but must pass by, content with directing your attention to another physical sign, heard at the time of the first examination, viz., "the clicking sound" accompanying the motions of the heart, and seated apparently in the pericardium; whether occasioned by the fluid, agitated by the heart's action within its envelope, by its impinging against the substance of the lung itself, or lymph effused on an inflamed pleura; or by the gradual separation of the semi-organised lymph from an increasing quantity of liquid, and the occasional contact of the opposed surfaces of exuded lymph, I feel totally incapable of deciding.

DR. MACARTNEY

ON THE

CURE OF WOUNDS WITHOUT INFLAMMATION.

To the Editor of THE LANCET.

SIR:—A sufficient time has now elapsed, since the publication of my treatise on Inflammation, to enable me to ascertain, through various channels, the manner in which that work has been received by the profession and the public. I have the satisfaction of knowing that both the principles and the practice advocated by me, have been adopted, and acted on, to the fullest extent, by those practitioners whose knowledge of the animal economy I the most respect; and that all the individuals *not* of the profession, who have experienced, in their own persons, the possibility of wounds being healed without inflammation, have the most perfect confidence in the treatment I have recommended, and are fully competent to carry it into effect. I have also discovered that many surgeons, without acknowledging the source from whence they derive their information, are adopting the remedial means by degrees; but in consequence of their not making themselves acquainted with the principles which could alone direct them aright, they commit many mistakes; for instance, in proposing to use the water dressing, the wet lint has been left without any covering, and thus, in a few hours, was converted into a dry application. In other cases, I have seen the injured limb in a depending position, or tightly bound up, or used in its accustomed movements; and in two or three instances I have known the oiled silk to have been placed next the skin, and the wet lint on the outside of it, that evaporation might not be impeded, as was said.

There is another class of practitioners, that I am happy to say is not numerous, who defend the hitherto universal opinion of inflammation being salutary, in all cases of open wounds more especially, although the arguments they employ are so futile, that one would be almost tempted to suppose that these adversaries are friends in disguise. It is necessary, however, for me to point out the invalidity of the objections which have been made to my theory respecting inflammation; and, in doing so, I shall omit the names of individuals, in order to avoid personal altercation, which would be unworthy of those who have to discuss a subject of such high importance to mankind.

1st, The main question at issue, and to which I shall confine myself at present, is,

Can inflammation ever be necessary, or even accessory, to the reparative and curative processes? As the negative reply to this question, it has been asserted, 1st, that an exalted, or irregularly-excited state of the vital powers is essential to reparative action, and that such excited condition only differs in degree from the most severe inflammation. I, however, deny that an increased sensibility and unusual dilatation of the arteries are necessary, or even desirable, in the healing of common wounds. The afflux of blood to a part, is in proportion to the quantity of new material required, and not in relation to the amount or nature of the injury sustained; and hence we find it most remarkable in certain natural and healthy processes of reparation, such as the growth of the new horns in the males of the deer kind, and also in the provisions made for the attachment and nourishment of the fœtus in the viviparous animals; and again, a mere augmentation of sensibility and a determination of blood, should not be confounded with inflammation, between which and them there is an essential difference, the former being characterised by the peculiar pain, the injurious effusions, and the sympathetic irritation of the constitution.

2ndly. Some who assume the necessity of inflammation, have gone so far as to attribute its existence to the lowest order of animals, and to plants; although, at the same time, they confess, that in these instances it is not attended with any of its proper phenomena. This is a mode of argument which does not admit of discussion; for, if we were to grant any premises that a person may please to assume, in contradiction to the evidence of our senses, our opponents might prove black white and white black, or any thing, however impossible. Besides, if we were sufficiently complacent to allow, that inflammation could exist without exhibiting any of its sensible effects, nothing would be proved in favour of inflammation attended *with sensible effects*. An invisible or polypine inflammation, which admits of the animal being cut to pieces, and turned inside out with impunity, would answer our purpose quite as well as none at all.

3rdly. It has been conceded by some, that inflammation is *essentially* a disease, while at the same time they contend that it conduces to the healthy action of repairing injuries. Mr. Hunter gave origin to this error, by confounding adhesion and inflammation; yet he insisted on the impossibility of two different *diseased* actions, existing together in the same part, and preserving their distinctive characters. We can readily admit the addition of common inflammation to a specific disease aggravating and altering the natural course of the latter, or two diseased actions modifying each other so as to produce a third, not perfectly resembling either parent; but I cannot, by the utmost stretch of imagination, conceive the possibility of any species of disease producing healthy actions, except in the manner of derivation; or that health and disease can ever co-exist, without interfering, or being in the relation of opponents the one to the other. As well might adverse winds and tide serve to propel a ship; or a watch, with all its internal mechanism deranged, most accurately mark the time.

4thly. I have heard it said, that other surgeons hold the same opinions that I do respecting the treatment of injured parts; that, after all, it will be found that we agree as to the facts, and only employ different words to express our meaning.

If this had been true, we should long since have had lacerated wounds cured, without pain, protracted suppuration, and erysipelatous inflammation; and punctured wounds, without repeated abscesses and incisions, long suffering, sleepless nights, and sympathetic fever; which are well known to be the frequent consequences of such injuries, when treated by the advocates of inflammation. All persons who have experienced the healing of an open wound, on the anti-inflammatory plan, are well assured that there is something more than a difference of words between the two theories, and the modes of treatment necessarily resulting from them.

5thly. As coagulable lymph is sometimes effused during inflammation, and generally as a consequence of injury; and as it is always designed to form the means of uniting divided parts, it might seem, at a first view, that this effusion is the product of inflammation; and, at the same time, is necessary to conservation and cure. Such has been the confident conclusion of those who hold the opinion, of inflammation being necessary to the healing of wounds. The co-existence or coincidence of two phenomena by no means, however, proves them to stand in the relation of cause and effect to each other. Neither does the precedence, as to time, of one event to another, show that the first is the cause of the second. All the circumstances must be taken into the account, before a correct judgment can be formed on this or any other subject. The conditions under which we observe the effusion of lymph to take place are three. 1st, When there is no appearance of inflammation whatever, as in the formation of the decidua uteri; 2nd, previous to the time that inflammation could arise, as immediately on the receipt of extensive incised wounds more particularly; 3rd, when inflammation has taken place, lymph is poured out to consolidate the cellular and serous tissues, and thus create a barrier, which will limit the inflammatory processes, or stop them altogether. Now, let us admit for a moment, that inflammation is the cause, and the effusion of lymph is one of its effects; in the

first case, I have mentioned, the effect would occur without the existence of the supposed cause; in the second, the effect would precede or prevent the cause; and in the third case, the effect would limit or arrest the progress of the cause.

Again, the cure of hydrocele has been exultingly quoted as the decided proof of the effusion of lymph being the product of inflammation, whereas, if we examine the matter fairly, the very opposite conclusion must be adopted; for example, in the cure by injection, the coagulable lymph serves to agglutinate the surfaces of the sac, and thus limits or prevents the inflammation, which would otherwise take place; and when the operation by incision, caustic, or the seton is employed, the cavity of the tunica vaginalis is obliterated, by lymph being converted into granulations; and when the inflammation has subsided, the contracting, absorbing, and healing processes complete the cure. It is, therefore, found, that lymph is always thrown out for the purposes of reparation; and when it fails in accomplishing this, it is because it is opposed by inflammation. It is evident that the great distinction between all the other effusions, which are the real effects of inflammation, and that of lymph, is, that the former are constantly injurious, and never contribute to the cure. It forms no objection, that a perfect cure cannot, under all the circumstances, be accomplished by the medium of lymph; for instance, when broken bones are so much displaced as to leave the limb deformed and almost useless, more lymph than usual is shed, and the processes of reparation are carried on with greater energy than if the ends of the bones were retained in exact contact.

I need not longer dwell on theoretic arguments. The important question, after all, whatever may be said on either side of the controversy, is simply this,—*can wounds of different kinds be cured in the human subject without inflammation?* No person, as far as I have learnt, has yet pretended to affirm the contrary, although some, who have never tried, have taken upon them to express doubts. I shall, however, go further, and assert, that there never was an instance of any wound being perfectly repaired or healed until inflammation had subsided, or in which it did not exist, and I challenge the whole profession to produce a single example of the kind.

I now take my leave of the subject, being quite satisfied, if my book had never issued from the press, that a change to a mild plan of surgical treatment would eventually arise, from more enlarged views of living nature, and as a part of the general improvement already made in the management of the human kind under all other circumstances. The custom of promoting the inflammation of wounds, before they were allowed to heal, has descended to us from those dark ages of the world in which insanity was treated by the whip and chains, when people were forced to profess their belief of impossibilities by the rack or the faggot, when the punishment of death was awarded to almost every crime, when wars were accompanied with the cold-blooded butchery of women and infants, and when the education of the young was conducted by the ferula and the rod. All these things are fast passing away. The humane spirit of the present age, and the universal spread of real knowledge, cannot fail to have their influence on the practice of surgery; and, at no very distant period, celebrity will be sought and obtained, not by dexterity in tormenting and carving the human body, but by scientific skill in the art of healing.

I address this paper to you, Sir, in the hope that you will give it publicity through the medium of your extensively-circulated periodical and am your very obedient, humble servant,

JAMES MACARTNEY.

UNGUARDED DIAGNOSIS—INVERTED UTERUS,

TREATED BY MISTAKE FOR GRAVEL.

To the Editor of THE LANCET.

SIR:—If you think the following of sufficient interest to occupy a place in your highly-prized Journal, I beg your acceptance of it. I am, Sir, your very obedient servant,

H. C. FIELD, M.D., M.R.C.S.L., &c.
Black Rock, near Dublin,
May 29, 1840.

It seems surprising, that replete as the medical journals are with valuable therapeutic and other scientific discoveries, they do not often lay before the medical public an exposé of the *errors* committed in diagnosis, or enforce the golden rule, that diagnostics are the most important branch of pathology. Pathological and therapeutic research might, perhaps, more rapidly advance, were the journals made less a vehicle for the marvellous, or the bright side of cases, and did they more frequently investigate the causes of failures. I have strong reason to believe that many valuable remedies, perhaps specifics, are declared useless upon trial, because they have often failed in the cases where applied, and that such failures are often mainly owing to the choice of cases to be treated with such remedies. Such a failure of diagnosis, I believe, occurs more frequently than we are aware of; some other disease over which they have no influence is simulated, or some preliminary is omitted, as I think cases, which I shortly

expect to publish, will justify me in asserting. For the present I shall only say, that interested motives powerfully operate in the healing art, to dissuade us from the publication of errors committed in diagnosis, while the multifarious publication of wonderfully successful cases is discouraging to the hope that any but the marvellous will be acceptable.

Feeling the force of this hypothesis, and unwilling just now to infringe upon it, I shall, perhaps, obtain a reading for my case by the novelty of its selection. I do not offer it as having a claim to originality, although it is the only one of the kind I have seen in a practice of *twenty years*, that was not the consequence of parturition, or of polypus uteri, but I do offer it as one, exhibiting conspicuously two points; first, the great advantage a practitioner has who is in possession of a knowledge of midwifery, over one, however eminent, who is unacquainted with it; and, secondly, the danger incurred by practitioners wishing to shorten the road to public favour, by endeavouring to be characterised for an off-hand dash in practice, as well as for delicacy and forbearance in the examination of female disease. The following case will exemplify that, by yielding to *misplaced modesty*, an irreparable mischief is sometimes done to the confiding patient, as well as to the reputation of the practitioner, whose quantum of success on such grounds may encourage him by habit, to the dangerous supposition, that he is gifted with a talent of making a safe diagnosis on a view of his subject as superficial, as in the following

Case.—I was called on at midnight, some time ago, to visit Mrs. ——, aged seventy, who said she was subject for years to painful attacks of gravel, and bearing down; that the present attack was the severest, as she had now, in addition, a painful swelling, and was in great agony, as, indeed, her piteous moanings indicated. Having mentioned a *bearing down* and swelling, I requested to be allowed to examine, on which she conducted my hand under the bedclothes to feel outside her shift; and on my *urging* to be allowed to examine more properly, I was informed, that the eminent practitioner whom she had consulted in all former attacks had never been so unreasonable, but from her description had always prescribed something for her. Being, perhaps, less punctilious than those who preceded me in her case, I urged that it was necessary to examine, was permitted, and found a tumour pending from the vulva, and extending between the thighs, very painful to the touch, and of the size of a new-born infant's head. There was a painful desire to make water, and at every effort the tumour received an *impetus* that seemed to swell and inflame it more. I tried ineffectually to pass the catheter, and return the tumour to its proper position within the os externum; but seeing that I had failed in both attempts, I directed fomentations, an emollient injection, and a draught of castor oil with laudanum, in expectation of soothing until my visit in the morning, at which time I succeeded in introducing the catheter, and giving exit to a *quantity* of *urine;* and subsequently returned the whole of the tumour, without any formidable difficulty: it was of an oblong, cylindrical shape, the greatest diameter being next the labia.

The absence of the os tincæ at the lower part, distinguished it from prolapsus; and its hardness, redness, and greater sensibility, together with the want of neck or peduncle, from polypus. I think I need scarcely say, this was a case of inverted uterus, mistaken, extraordinary as it may appear, during its incomplete stage, for gravel. In consequence of which mistake she had been for years dragging on a miserable life, for want of that aid, the necessity of which a more minute investigation would have made apparent. I am happy to say, the complaint, mistaken and treated as a case of gravel, for the space of twelve years, has now, even at her late period of life, been perfectly relieved, simply by the use of a globular pessary.

DISEASE OF THE CÆCUM AND APPENDIX VERMIFORMIS,

ISSUING IN A FISTULOUS OPENING THROUGH THE ABDOMINAL PARIETES.

To the Editor of The Lancet.

Sir:—The following case occurred in a patient admitted into the Manchester Royal Infirmary, whilst I officiated as clerk to Dr. J. L. Bardsley. If you consider it worthy of publication, in connection with the recent investigations on cæcal disease, perhaps you will be kind enough to insert it in your excellent Periodical. I am, Sir, your obedient servant,

G. Southam, M.R.C.S.L.

Salford, Manchester.

June 26, 1840.

May 15, 1835. Thomas Ryley, aged 22 years, weaver, stated that twelve months previous to his admission he received a severe blow on the lower part of the abdomen, to which he attributed his present affliction. It commenced with pain in the abdomen, at first slight, and not incapacitating him from following his employment during the first six months of its existence. From this period it became more violent, and a tumour gradually developed itself on

the right side, immediately above Poupart's ligament.

When admitted, the abdomen was very tender, and an obscure tumour could be felt on the right side of the linea alba above the groin; it was very firm, and pressure upon it caused pain. His bowels were moved daily, the fæces being generally light coloured and loose; the swelling continued to extend upwards until the 23rd of July, when it burst at the umbilicus, and a considerable quantity of fæculent matter of a light colour and thin consistence was discharged; this gave immediate relief to the pain which had accompanied its formation. Fæces were discharged almost daily from the umbilicus, as well as from the anus, the quantity varying in proportion to the violence of the diarrhœa which was present. He died about the middle of September, and on examination the following appearances were detected:—

The lower portion of the ileum and the cæcum were extensively thickened in consequence of lymph having been thrown out on some previous occasion. A sinus was found extending from the cæcum to the umbilicus; it was formed by the appendix vermiformis cæci, which was bent upwards and adherent to the anterior parietes of the abdomen, to within about an inch of the umbilicus, with which it communicated by a short fistulous canal. The appendix was thickened and dilated, and the shell of a hazel nut was impacted in its middle portion.

CARIES OF THE STERNUM WITH ULCERATION OF THE LUNG.

Sir:—As the following case may present something of interest to many of your readers, I shall feel obliged by its insertion in your valuable Journal. I am Sir, your obedient servant,

G. Coward.

Tiverton, June 22, 1840.

ULCERATION OF THE LUNG, ORIGINATING IN CARIES OF THE STERNUM.

A full, powerful man, but of a decidedly strumous diathesis, was affected with circumscribed inflammation over the sternum, and oppression in the chest, about three months since; symptoms which he attributed to an accidental chill. The local appearances at this time were circumscribed œdematous tumefaction; heat, pain, and redness, situated opposite the mamma a little to the right of the mesial line. The patient complained of deep-seated pain and oppression beneath the externally affected part; there was cough, dyspnœa, and slight expectoration. The general symptoms were febrile: quick, hard pulse; hot skin, thirst, white tongue, and constipated bowels. As the morbid process proceeded, the existence of pus became evident, and the swelling increased, with fluctuation, whilst the general symptoms assumed the hectic type. By the application of poultices the progress of the pus to the surface was facilitated, and after some days the abscess spontaneously broke, discharging ill-conditioned and offensive matter. The sternum was found to be carious: it was rough and excavated, and continued to discharge offensive pus as the morbid process increased. Notwithstanding active and judicious treatment, the disease continued to extend until the sternum was completely perforated and the lung exposed, which had doubtless contracted extensive adhesions by the opposing surfaces of the pleuræ during the increase of disease. The lung itself now became affected: a large ulcerated cavity was soon formed (most probably in the middle lobe of the right side), and air escaped easily by any respiratory effort. In this condition, with but slight aggravation of symptoms, he still continues, and at present he presents the following morbid signs, local and general. There is an ulcerating cavity at the point indicated, of about two or three inches in extent, through which air readily escapes in any powerful respiratory effort, but not during ordinary breathing; the margin of this cavity is red and tumid, and there is considerable tumefaction over a similar spot on the left side. The patient complains very little of pain, but the inflamed portions are extremely tender. There is occasional cough and expectoration, and the fever present is hectic: there is a permanently quick pulse, exacerbations of fever occurring in remittent paroxysms, occasional night sweats and thirst, whilst the strength fails rapidly; but there is no emaciation, as of late; the countenance has become puffy and œdematous, and of a leucophlegmatic aspect; and there is a decided tendency to general anasarcous effusion, as is evinced in the appearance and feel of the general surface.

Judging from the negative effect of treatment, the strumous diathesis, and the steady but decided advance of bad symptoms, the prognosis is unfavourable; whilst, on the contrary, the patient's youth, usual strength, and active habits, are in his favour.

Health of the Army.—In the Supplementary Estimate of the Office of Ordnance, 1840-1841, laid, on the 1st instant, before the House of Commons, the sum of £2781 is proposed to be expended in the erection of Ball and Fives Courts in the Barracks of Great Britain, under the authority of a Treasury resolution, passed in April last.

MR. NASMYTH'S REPLY TO MR. OWEN.

To the Editor of THE LANCET.

SIR:—I lose no time in undertaking the "explanation" and "justification" of my views on dental development, but before entering upon that duty, allow me to make an observation with respect to the separate publication of the abstract of my papers, read at the British Association, and contained in the forthcoming volume of last year's "Transactions." Had I been aware that such a cunning outcry would have been raised on what I shall proceed to show is altogether a groundless assumption, I certainly, for my own sake, should not have sent you that abstract for review. It was the most convenient and concise, as well as the only corrected document, for me to refer to on the subject; but my opponent has reaped far more advantage from it than I have, by attempting to raise a prejudice against me with respect to it. That that prejudice is utterly unfounded, my conduct will at once show; for so far from being forced to rely solely upon this abstract in "justifying" and "explaining" my views of dental development, I have really no occasion to refer to it at all, and shall proceed to my task with, simply, the reports published last year in the "Literary Gazette" and "Athenæum," before me. I may remark, however, that I had copies of the abstract for some time in my possession before I sent one to you, and that I had received repeated assurances from Mr. Phillips, in writing, that I was at liberty to give publicity to my views on the subject of dental development in any way I thought proper.

My motive for sending you a copy of it for review, was very natural; I wished you to compare my own account of my researches with Mr. Owen's history of his "Nouvelle Theorie," in order that you might not have to complain, that you were only made acquainted with my views, through the report of a third party. The report of my papers, given in the "Literary Gazette," was not my own, whilst that which I sent to the "Athenæum" was so altered and abridged, that I cannot be considered responsible for it. However, I have not the slightest reluctance to challenge Mr. Owen to a comparison of our respective pretensions, even under the disadvantage of quoting from hasty reports of my own papers, made or modified by other persons; whilst he is at liberty to appeal, in his defence, to a statement, carefully drawn up by himself, after my views had been laid before two different sections of the British Association (of which he is one of the Council, and was present at the meeting), and after the publication of the reports of my papers. With respect to my "Abstract," I at once state that it contains no interpolation whatever of matter not contained in the "Literary Gazette" and "Athenæum," and I am therefore quite at a loss to divine why I am not justified in having sent it to you for review. I conceive, on the contrary, that I was only doing bare justice to myself by taking this step.*

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* As Mr. Owen has published in another journal, as well as in THE LANCET, a long and rambling letter to Professor Phillips, charging me with introducing matter into my "Abstracts" of the papers read at the British Association, which was not contained in those papers themselves, allow me to extract here, my reply to his accusation:—

"*The abstract of my papers given in the 'Transactions' of the Association contains no interpolations whatever; no matter which was not contained in the papers themselves:* I wish the same could be said of all Mr. Owen's reports published in the same work. The promptness with which that gentleman at once accuses others of the practice of interpolation, appears to have its origin in the circumstance, that he is far from being unfamiliar with that practice himself. I should advise him, before he again 'prays' Prof. Phillips to compare my abstract for the 'Transactions' with the reports in the 'Literary Gazette' and 'Athenæum,' to request the secretary to institute a similar comparison between his own abstract, in the 'Transactions' published in 1839, and the reports given in the weekly journals, shortly after the meeting of the Association in 1838. Prof. Phillips would, by so doing, be able to make out a case of interpolation so flagrant, that all Mr. Owen's disinterested indignation against such proceedings would be perfectly explained. In the eighth volume of the 'Reports' of the British Association for the year 1838, p. 136, the following passage occurs in the abstract of Mr. Owen's paper:—

"'Purkinje and Fränkel also added to dental anatomy several new and interesting facts relating to the structure of the enamel, pointing out, more especially, the form and characteristic transverse striæ of the component crystals: and lastly, they determined the true osseous nature of that distinct layer of substance which had been previously known to surround the fang in the teeth of man, and which they once observed to be continued on the enamel of a human incisor. *This observation Mr. Owen proceeded to state he had confirmed, and he exhibited several sections of the simple teeth of the mammalia, in which both the ivory and enamel were invested by a layer of osseous substance, identical in its structure with the cement which enters more abundantly into the composition of the compound teeth of the herbivora.*'

Indeed, a more absurd attack than Mr. Owen's accusation against me can scarcely be conceived. A paper is read by me in August, 1839, and because I dare to distribute an abstract of it *so early* as June, 1840, I am to be the object of a tirade from an individual who, between the reading of my paper and the publication of the report of the Association, has been writing on the same subject in analogous terms in two countries, and, if I mistake not, in more than two places.

One other introductory observation, Mr. Editor, you must also permit me to make in reply to an impertinent insinuation of Mr. Owen. He would, it seems, have his readers believe, that I am the author of the review which appeared in your Number for June 6th. Now, I beg to state, and you will bear me out in the assertion, that the article in question was not written by me, and that I did not see a line of it before it was published.

In answer to the first charge brough against him, that he had incorrectly stated recent writers to have regarded the teeth as inorganic, Mr. Owen replies, that he used the word "inorganic" in its *German* sense. This is, doubtless, a very subtle answer, but it labours under the disadvantage of being very little to the point; for in my papers, as reported in the "Literary Gazette" and "Athenæum," I described the teeth (as I shall shortly show) to be formed by a process of ossific "transition," and, consequently, as not being inorganic in any sense

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"Not a word, however, of all this is contained in the lengthy report of Mr. Owen's paper printed in the 'Athenæum,' which *he* acknowledged to me was furnished by *himself*, nor in the 'Literary Gazette;' and for the best of all reasons, viz., that at the time when his paper was read, and the 'Athenæum' report of it published, Mr. Owen had *never seen* the work of Purkinje and Fränkel; though in his report, he would fain lead the reader to believe, that he had been well acquainted with these writers in the autumn of 1838. The following letter to me shows, that he did not even receive a copy of their work till the spring of 1839:—

"'April 1, 1839.

"'My dear Nasmyth,—I received from Baillière, on Saturday last, the inclosed copy of Fränkel,* and as I shall not have time to look into it till the end of the lectures, I send it to you, knowing your anxiety to see it. I may ask for it again towards the end of June. * * Believe me, dear Nasmyth, yours very truly,

"'R. Owen.'

"After perusing this, I think you will agree that Mr. Owen is not exactly the person, in whom it is safe policy, to write a letter to Mr. Phillips, containing unjust accusations of interpolation against another party. Mere interpolation, however, although not to be commended, is a matter of no great consequence, unless it be adopted from some reprehensible motive. Let me state what was the motive of Mr. Owen in this particular instance. It can have been no other than that of appropriating to himself a discovery to which he had not the slightest claim, as I shall prove even by his own written admission. By his own report of his paper, read at the British Association in August, 1838, we learn, that he then made no mention of the enamel capsule, nor did he of the researches of Purkinje and Fränkel, for the very valid reason communicated above. Shortly after his return from Newcastle and the Continent, however, I sent him a paper containing an account of my discovery of the enamel capsule, which was afterwards published in the twenty-second vol. of the 'Med.-Chirurgical Transactions.' This paper he returned to me with the following note, dated

"'December 11, 1838.

"'Dear Nasmyth,—Hearing that you had made inquiries about the MSS., I return them without delay. I have marked in pencil all that occurred to me in the way of amendment, and I think it will be an acceptable paper for the Medico-Chirurgical Society. I am, in haste, faithfully yours,

"'R. Owen.'

"In April, 1839, he obtains the work of Purkinje and Fränkel, and then, on preparing the abstract of his papers read at the Association in the month of August previous, he inserts a passage, in which he represents himself as having there stated that he had confirmed and followed out their researches, and anticipated my discovery of the existence of a capsular covering external to the enamel.

"It is a great pity, as I have before hinted, that Mr. Owen, who, in the year 1840, displays such virtuous zeal, in writing to Mr. Phillips to prevent the reports of papers for the 'Transactions' from being modified, so as to comprise an account of discoveries made since those papers were read, did not manifest the same laudable anxiety in the year 1839, since, at present, his ingenuous cries for justice are quite uncalled for; whereas, last year, had he prayed Professor Phillips to compare the proofs of abstracts for the 'Transactions' with the reports published at the time, in the hebdomadal journals, I might have been spared the pain of exposing what, I think, must appear to all impartial eyes a very equivocal transaction, more especially as it was perpetrated under the guise of friendship."—*Med. Gaz.*, No. 40.

* In which Purkinje's researches are published.

of the word, German, French, or English. I, therefore, maintain that Mr. Owen's assertion, that recent writers, without even excepting me, had regarded the teeth as inorganic, is decidedly incorrect.

Men are generally most violent where they are most in error: at any rate, no where in Mr. Owen's letter is he more abusive than where, labouring under a mistake himself, he tries to fix a charge of plagiarism upon me. He accuses me of gross injustice to recent writers, and to Retzius more particularly, in claiming the discovery of the cellular structure of the interfibrous substance. Now the fact is, the researches of Retzius, which he is pleased to assume I have appropriated, only go to establish the existence in the ivory of osseous corpuscles, principally, towards its periphery, on which, he states, the tubes ramify, and in which some of them terminate. The interfibrous spaces he does not expressly describe, further than by stating that ramifications of the tubes course across them. Of what is further contained in them he has actually given no account.

In support of his altogether unfounded and unaccountable assertion, Mr. Owen, in order, apparently, to impose on the gentle reader, parades a reference to Baly's translation of Müller, Part I., p. 427, 2nd edition; but if the reader, in conformity with his recommendation, should consult this same translation, Part I., p. 427, 2nd edition, he will there find no mention of any intertubular substance at all, much less of its being structureless, or the contrary.

How, therefore, can I be an "unblushing plunderer" of Retzius, in describing these spaces as being filled with ossific substance of a cellular structure, which he has scarcely mentioned, except to mark their extent? Purkinje, whom I am also accused of injuring, by announcing as my own, the discovery of the cellular character of the interfibrous substance of the teeth, actually believes that "close as the fibres are together the *structureless* (*structurlose*) intermediate substance still forms the greatest part of the mass of the tooth."* This passage is extracted from Müller's review of the progress of anatomy and physiology in the year 1835; and as he expresses no dissent from the statement it conveys, the natural inference is, that he agrees with it. In what way, then, have I been unjust to, or how have I plundered from Purkinje and Müller, by announcing as my discovery the cellularity of the interfibrous substance? Mr. Owen states that Müller discovered the fibres of the ivory to be tubes, this is not correct; Müller himself attributes the discovery to Purkinje. It were really to be wished that Mr. Owen would study a question before he takes upon himself to write on it dogmatically, and that he would be still more careful not to make ignorant accusations which can only redound to his own disgrace.

With respect to the pulp, I claimed no discovery in my papers in regard to its internal organisation, though I think my researches were more minute than those of previous anatomists. I stated in August that it was mainly composed of "a number of minute cells in a vesicular form," and invested by a "reticular" membrane. On the 16th of December, Mr. Owen states, in his history of a "Nouvelle Theorie" of the formation of the teeth, that it consists of "semi-opaque polyhedral granules, or cells suspended in a clear matrix, and the whole inclosed in a tough transparent membrane, which forms the outer surface of the pulp."—*Comptes Rendus*, p. 786. Both these descriptions were made with a view to facilitate the demonstration of the process of dentification. I leave the analogy between them to be judged of by your readers.

Mr. Owen claims the discovery that the teeth are not "bodies of an inorganic nature, formed like brute bodies, by the juxta position of layers successively exuded by a glandular pulp or membrane," which, he says, is the theory of previous writers, *myself included*; but that they are formed by the transition of the pulp itself into ivory, by a "centripetal" deposition of ossific matter in its cells. He tries to make out that I am in favour of the former doctrine, and can, consequently, lay no claim to his "Nouvelle Theorie," by omitting, in his reply, all the passages in the reports of my papers which are adverse to his purpose, and by ingeniously selecting all expressions of the reporter, which, when isolated, seem or can be made to bear a meaning favourable to his object. Now let me show, by extracts, what my papers really do say, for what they do *not* say, Mr. Owen's account may be very advantageously consulted; first, and foremost, let me observe that I, according to Mr. Owen, a supporter of the old theory of exudation, in opposition to that of transition, was at the unaccountable pains, last summer, of having made a great number of large diagrams *expressly for the purpose of showing the cells of the pulp in their ossific transition into the cells of the ivory*.

The original sketches of these diagrams and drawings had been previously, for a considerable period of time, accumulating under my hands, as specimens occurred in my researches from which they could be made. When the collection had been completed by my excellent and talented artist, Mrs. Holmes (who, as well as several other persons, can prove, without the slightest difficulty, that it was in existence some time be-

* See Müller's Archiv for 1835, Part II., and also a notice of the passage quoted above in my "Researches on the Teeth," published last year, p. 46.

fore the meeting of the British Association), I took it with me to Birmingham, and there exhibited it in illustration of my papers. Had I been in favour of the old doctrine of dental formation, I should scarcely have written a long paper on the subject, and should certainly not have gone to the trouble and expense of having a large collection of illustrations, made in confirmation of previous views. Moreover, as will be seen by referring to the "Lit. Gaz.," I stated that my theory was both "bold and novel," though Mr. Owen ridiculously endeavours to show, that my descriptions differ in no respect from those of previous writers. The diagrams above mentioned I have since exhibited at a lecture delivered by me at the Royal Institution; and they are all, as well as the drawings, still in my possession.

In order to examine the pulp for the purpose of studying the process of dentification, the ivory already formed at the surface of the pulp (for the transition goes on gradually from the circumference towards the centre) must be forcibly separated from the unossified portion beneath. When this has been done, the question suggests itself, whether the ivory already formed, fragments of which have, of course, in the disruption, been left strewed on the surface of the pulp, has been secreted or exuded by the pulp; or is, on the contrary, an ossific transformation of the latter. I have shown that I am altogether in favour of its being an *ossific transformation*; the words "excretion" or "exudation" never occur in my paper, any more than the corresponding ideas did to my mind; on the contrary, I said that the cellular fragments found upon the surface of the pulp "*are in size and appearance perfectly accordant with the cellules of the pulp*;" the only object of which observation was, of course, to prove that the latter are converted into the former; and I went on to state that, at an early stage of development, "the different layers of cells will be seen, and *the transition into ivory observed*."—*Lit. Gaz.* for Sept. 21, 1839, p. 598. Here I exhibited no fewer than 50 drawings before the medical section, illustrating this process of transition. In describing the process of ossific transition in the very cells thus depicted, I am represented by the reporter as having stated, that "when merely a thin layer of ossific matter has been deposited on the surface of the pulp, it may with great facility be drawn out entire." Mr. Owen fixes triumphantly on this passage to show that I am in favour of the old theory of exudation, though this observation is made of a drawing to which I was actually pointing at the time, to show the appearance *of the transition of the cells to an ossified state*. The fact is, when the external layer of the pulp becomes ossified, it can no longer be regarded as the pulp. It is then spoken of as a layer of ivory in apposition to, and in connection with, the surface of the pulp beneath. Thus the deposition of ossific matter on the surface of the pulp simply means its deposition in the cells of the reticular, formative surface, which is undergoing the process of dentification. That the idea of exudation could not even have been floating before my mind, is proved by a subsequent passage, where I state, "*that the manner in which the osseous matter is deposited in the cells of the interfibrous substance, I had not been able to discover.*" It argues no little assurance, to say the least, on the part of Mr. Owen, to assert, after quoting this sentence of the date of Sept. 21, which he does, that I was ignorant of, and, indeed, opposed to, the theory of the formation of the tooth by ossific deposition in the pulp, when I read my paper; that I altogether changed my views on the appearance of his memoir of the 16th December, and that I then borrowed it from him. He surely would not venture to make such outrageous assertions, if he did not calculate on his position and influence for carrying him through any controversy with impunity, however unfairly it might be conducted on his part.

In the "Athenæum," No. 620, p. 707, I said, that the "cellules of the fragments of the ivory which are found scattered on the pulp, *resemble exactly in size and appearance the cellules of the latter, when in a state of transition.*" It is quite futile for Mr. Owen to argue on the face of these passages, that I am in favour of the old theory of exudation, simply because now and then I speak of the ossified portion of the pulp as a layer of ivory in apposition to the formative or transitive surface of the yet unossified pulp beneath. With equal, if not more, reason, I might infer that he is a supporter of the old doctrine, from a passage of his, at p. 504 of the "Medical Gazette," when he speaks of the "well-known facility with which the layers of ossific matter can be detached" from "the surface of the pulp." I, also, in opposition to former writers, hold, that the layers of ivory are not merely in apposition to, but are also in organic connection with, the pulp; not only are all my observations on the process of transition in favour of that view, but the fact that, after the forcible separation of the ivory from the yet unossified pulp, cellular osseous fragments are found strewn over the latter, shows that they were organically connected, and the disjunction positively confirms it. It does not suit, however, Mr. Owen to admit this, and so he roundly asserts that I regard these cells as excreted, although neither that nor any similar word occurs in the lengthy report of my papers. So much for his fairness and conscientiousness! He perverts my meaning where he can; and when perversion fails to effect his object, he instantly draws upon his imagination, and attributes to me the first phrase that occurs to him, which it

would suit his case for me to have uttered. Besides, who ever heard of the exudation or excretion of ossified cells? the idea of such a physiological process is an absurdity; and if his case were not desperate, you may rest assured that Mr. Owen would find a better means of escape from the charge brought against him, than by attributing to me such inconceivable nonsense.

Mr. Owen's defence of the originality of his discovery of the correspondence between the granules, or cells of the pulp, and the fibres of the ivory, appears to me to be very lame. I had clearly stated the identity of the dental fibres with the fibres of the surface of the pulp; but he tries to represent me as a supporter of the old theory, because the reporter of the "Literary Gazette" uses a passive, instead of an active verb, in describing my observations. His interpretation is so laboured, as to be a curiosity in the annals of sophistry. Finding in the "Literary Gazette" the phrase, "that the framework of the reticulations or cellules of the pulp is constituted by the fibres of the tooth;" although it is evident, that to express properly the meaning of the phrase, "constitutes" should have been used instead of "is constituted by," he does not hesitate to assert, from this inaccuracy of the reporter, in opposition to the whole tenor of the papers, my meaning to be, that the fibres of the teeth, after having been excreted or exuded by the pulp, produce fresh fibres of the same diameter, and similarly curved, on the surface of the latter; he would fain make your readers believe, that I have described the fibres of the reticular surface of the pulp as being merely the impression from the fibres of the superimposed ivory; though in the "Athenæum" (No. 620, p. 707) I have expressly said that, "from the spirally fibrous framework of the reticulations are evolved the spiral fibres of the tooth;" by which evolution it would be ridiculous to suppose that I meant any process resembling "excretion" or "exudation." I merely wished to state, that at the formative or reticular surface of the pulp, where, to use my own words, "the osseous matter is deposited in the cells of the interfibrous substance," the fibres themselves, by a process of evolution or development, are converted into the fibres of the ivory. I do not deny that Mr. Owen's descriptions may be clearer than those ascribed to me, for he drew them up himself, at his leisure, whereas my reports are the hasty productions of a third party; but if the reports in the weekly journals are to be thus unfairly dealt with, men who cannot make science their sole and exclusive occupation, had better retire at once from the British Association, and leave to figure there those gentlemen only, who, like Mr. Owen, have time to publish their "nouvelles theories" whenever they please, and who do not scruple to interpret just as it suits them, and to take advantage of hasty reports.

You alluded in your article to the analogy which existed between that portion of Mr. Owen's memoir, in which he says that he had been unable to recognise the "precise arrangement of the hardening salts" in "the external membrane of the pulp;" and the following passage in the report of my paper, given in the "Literary Gazette:"—"The manner in which the osseous matter is deposited in the cells of the interfibrous substance he (Mr. N.) had not been able to discover." In his reply, Mr. Owen says that his observation refers to the external membrane of the pulp, whereas mine relates to the interfibrous cells. This is mere shuffling; for the interfibrous cells, to which I allude, are those of the external surface of the pulp, inasmuch as it is there alone that the process of ossification is carried on. His other observation on this subject is a petty quibble. It is not, forsooth, the "manner" in which the osseous matter is deposited that has escaped him, but it is the "arrangement" of the same, when deposited, that he has not been able to "recognise," as if the one did not imply the other. Had he "recognised" the manner of deposition, he would have been acquainted with the arrangement; and if I had been able to discover the arrangement, I should have obtained a clue to the manner of deposition —we were speaking, therefore, in reality, of the same thing. Paltry as is Mr. Owen's defence, he assumes quite a triumphant air after having made it, and thinks himself privileged by his success to storm at you, Mr. Editor, and to throw out an emphatic insinuation against me, which is as false as an "emphatic assertion" represented to be mine, which, in another place, he has pretended to quote from my report, *but which is in reality, as has there been shown, one of his own fabrication.**

* "At p. 507 of the last Number of this Journal, he (Mr. Owen) manufactures what he is pleased to call Mr. Nasmyth's 'emphatic statement' of September, 1839, 'that so far from being the ossified pulp, it (the dental substance) was altogether a distinct formation.' In making this fabrication, he appears to have been assisted by a friend; and, in order to give it greater weight, he takes the trouble to inform us that the character of that friend 'is the very antipodes of impudence and duplicity.' Now of this passage which he gives, as quoted from the 'Lit. Gazette,' p. 598, only *the last four words will be found*, on reference to that journal, to be correctly copied: *all the first part of the passage is the composition of Mr. Owen.* Mr. Nasmyth, by this perversion of his report, is made to say that the dental substance is altogether a distinct formation, whereas he never mentions the

The passage which Mr. Owen has thus misquoted, and upon which he principally relies for making out his case against me, occurs in the report of the "Lit. Gazette," *after I have finished the exposition of my own views*, and where I am speaking of those of Schwann. This author regards the pulp as simple cartilage, and as being directly converted into bone; whereas my researches tend to show that by a distinct formative process carried on at the surface, the vesicles of the pulp are converted into cellules, previous to the deposition within them of osseous matter; and hence the reporter for the "Literary Gazette" was quite justified in stating that "Mr. Nasmyth regarded the cells of the ivory as a distinct formation."

Certainly he is the last person from whom one would expect to hear a boast which is meant to reflect discourteously on the character of his adversary,—that *he* possesses friends "who are the very antipodes of impudence and duplicity." The matter can only be explained by supposing that his ideas of what constitute modesty and candour, are the very antipodes of those generally received in this quarter of the globe.

In the reply to the charge which you very justly brought against Mr. Owen, that he exported beyond seas, and first published in a foreign language, his so called "*Nouvelle Theorie*," he asserts that the theory of dental development, propounded and supported by the researches described in his memoir, were first dutifully submitted to the members of the Royal College of Surgeons, in his lectures on the teeth, delivered in May, 1839. You will please to observe, Mr. Editor, that Mr. Owen, who has attempted to excite a very gratuitous opposition to my claims, as represented by an "*Abstract*," which has been published and in my possession for two months, because the volume in which it is contained, and which Mr. Owen with marvellous assurance denominates a "nonentity," is not yet out, here appeals, in his defence, to a series of lectures *which have never been printed or published at all*, and which, perhaps, only exist in rough manuscript and notes. Pray observe, also, that though I, myself, am not to be allowed to give any evidence, or be any authority on the theory propounded by me at the British Association, but am to abide patiently and implicitly by the account of it furnished by the reporters or editors of the "Literary Gazette" and "Athenæum," Mr. Owen's word is, it seems, to be taken at once, as to what he did or did not state in his oral dissertations of 1839. The injustice here done to me I need not point out; it is perfectly accordant with the line of conduct which Mr. Owen has for some time past thought proper to adopt with regard to me; and it is only one of the many proofs I have received, that in the discussion of this question by Mr. Owen, he will not be at all scrupulous in regard to what arguments he has recourse to.

But, strong in the justice of my cause, I will still maintain my ground, though non-extant lectures and researches taken for granted, are appealed to against me; though, by the reputation and influence of my opponent, it will, of course, be sought to extinguish me; though the Secretary of the British Association is already summoned to denounce me; and though, doubtless, the National Institute of France will next be required, in gratitude, for the honour done it by the publication of *nouvelles theories*, in the "Comptes Rendus," to contribute to the demolition of the daring pretender by whom the originality of these theories is impertinently disputed. But to return to the immediate question:—Is it at all probable that Mr. Owen would propound in his lectures an important and entirely new theory on the development of the teeth, without recording it in some publication, so as to establish, beyond dispute, his claim to it, and prevent its being appropriated by another? How came it to pass, that this theory, represented to have been thus liberally laid before the scientific world, remained, so to say, dormant for eight months, and was then given as a "*nouvelle theorie*" to the French Institute? Above all, and I particularly recommend this question to the cogitation of Mr. Owen, and to those who, in conformity with his recommendation, have turned to Dr. Baly's translation of Müller, in their examination of this question,—how is it, that, in the second edition of that work, Dr. Baly did not incorporate the important discoveries stated to have been announced in May, 1839? Mr. Owen has the pleasure of Dr. Baly's acquaintance, which I have not, and would, doubtless, mention such discoveries to him, as he actually did with respect to his researches on the kidney; he also, of course, knew, which I did not, that a second edition of Part I. of Müller's "Physiology" was in preparation, and could not, naturally, be otherwise than anxious that it should not appear without recording the "nouvelle theorie" to which he had recently given birth. I have been long unremittingly engaged in researches on the teeth, and had such a theory been clearly announced, or even roughly sketched, either in writing or by graphic delineations, by Mr. Owen, in his lectures of the spring of last year, I could not fail to have heard of it; I did not, however, attend these lectures myself, and, therefore, can furnish no direct evidence on the subject; but immediately after perusing Mr. Owen's reply to your

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dental substance at all; and his real meaning evidently is, as we have shown above, that the cells of the ivory have undergone a distinct formative process in their transition from the cells of the pulp."—*Med. Gaz.* No. 43, p. 596.

article, I wrote to a friend, who had heard the whole of them, a letter of inquiry, and received from him the following reply :—

"June 29, 1840.

"My dear Sir:—In reply to your inquiry, I beg to inform you, that in the course of lectures delivered at the College of Surgeons in 1839, I do not remember that Prof. Owen made any mention whatever of the new views of the development of the teeth by an ossification of the pulp. I may here state that he did allude to the enamel capsule; in so doing he mentioned your name, but in a manner that led me and others to believe that you had not discovered the capsule, but that he himself had done so; he added, that the latter discovery was the most interesting that had been made in this branch of anatomy since that of the 'tubular system.' Yours, &c.

"Alex. Nasmyth, Esq." "* * *"

From this evidence, and also from that furnished by another friend, who has written to me on the subject, in equally conclusive terms, as well as for the reasons above detailed, I must be allowed at present, Mr. Editor, to question the accuracy of Mr. Owen's assertion, that he announced his "nouvelle theorie" of December 16, in his lectures of May.

Mr. Owen has accused me of "versatility," and has complained of the injury which the prosecution of my claims may inflict on the sale of his "Odontography." His accusation I have repelled, and to sympathy on the score of injury I can certainly prove that I have a far juster claim than Mr. Owen. To show your readers with how bad a grace such charges and lamentations come from Mr. Owen, allow me to extract from a letter, published in another place, what I have been compelled to state on this subject. The reader will soon see how coolly my interests have been sacrificed by the versatility of Mr. Owen.

"Mr. Owen appears to possess certain qualities to such a remarkable extent, that he cannot imagine them absent in others: for instance, he accuses me, throughout his letter, of versatility—a quality with which I have never, to my knowledge, been before reproached, but which I am quite sure, and shall immediately proceed to render evident, he possesses in no trifling degree. In the summer of 1838, I prepared for the press a translation of the work of Professor Retzius, of Stockholm, entitled, "Researches on the Microscopic Structure of the Teeth," and advertised it for publication; this translation Mr. Owen borrowed of me in the course of the same summer. Before advertising its publication, I had inquired of Mr. Owen whether he intended to publish anything on the subject. He replied in the negative. However, as he continued to retain my manuscript of the translation of Retzius in his possession, and as other circumstances had led me to believe that he might, nevertheless, entertain the idea of writing on the structure of the teeth, in which case I should with pleasure, as I told him, have abandoned my intention in his favour, I repeated the question in a letter, to which I received the following reply :—

"July 25, 1838.

"'Dear Nasmyth :—Many things have interfered to prevent my returning you the translation of Retzius earlier, but as you have not sent for it, I hope without inconvenience to you. *As I have before said, I have neither desire nor object in bringing before the public any of the general observations on the structure of the teeth, which I once hoped were new; but now perceive to be mainly anticipated by the industrious and sharp-sighted Swede.* It will obviously, however, be a source of great credit, and a matter of importance, to whoever practising in the line of dental surgery should combine these discoveries with the *practical* or *remedial* part of the science.† * * * * * * * * * * * * *

"'Believe me, dear Nasmyth,
ever yours,
"'R. Owen.'

"It will scarcely be believed, but it is no less a fact, that, in spite of this letter, and of my having, in consequence of Mr. Owen's repeated assurances, advertised a work on the subject of which the translation above alluded to was to form a prominent part, that gentleman, within about one calendar month after the date of the above epistle, not only published all the new 'general observations on the structure of the teeth,' but also all the 'practical' deductions from them which he was capable of drawing; that is to say, besides the anatomical details, he laid before the Geological Section the deductions from them bearing on natural history generally, which I had already alluded to in my advertisement; and before the Medical Section, he treated the subject, combining these discoveries with the practical or remedial part of the science.' For proof of this, the reader has only to refer to the 'Athenæum' of September 1st, 1838, and to the 'Literary Gazette' of September 15th, 1838, where the 'elaborate and voluminous' reports of his papers, read at the British Association, on the structure of the teeth, are published. What renders this proceeding still more remarkable is, that he gave his 'analysis of the laborious and accurate microscopical observations of Professor Retzius, as related in the original Swedish memoir of that author' (vide 'Athenæum'), a memoir with which he was only

† The remainder of this letter I omit, as it contains matter which Mr. Owen wished to be considered confidential.

acquainted through the translation he had borrowed of me, which he had never asked permission, and had, therefore, no right to make use of, and which he had returned to me, as the reader has seen from the above note, with an assurance 'that he had neither desire nor object in bringing before the public any of the general observations on the structure of the teeth.' I think your readers must allow that I have now established my point, and that Mr. Owen is, indeed, versatile with a vengeance.

"The appearance of his 'voluminous' reports naturally vexed me not a little. I was indignant, that a person, calling himself my friend, should publicly make use, for his own advantage, and without my leave, of a manuscript which I had prepared and advertised for publication; but, nevertheless, I remained silent on the subject, because I hoped that his interference was now, at any rate, at an end, and that from him I had no further competition to expect, as he, indeed, gave me to understand was the case. However, I was disappointed. Having once broken his promise, he was not to be deterred from aggravating the offence. It would be wearying your readers were I to follow him through all the evasive windings of his course, from the time when he had 'neither desire nor object to publish,' to the day when the advertisement of the first part of a bulky work on the subject issued from M. Baillière's shop. Suffice it to say, that, at first, he was 'persuaded to publish'; then he was 'determined to publish,' but only on a limited scale; until, finally, he resolved not only to give to the world the whole subject, and nothing but the whole, but also to render his work 'as generally useful as possible.' Throughout his whole correspondence with me, he has shown himself to be nothing, if not versatile—to use no harsher term."

I must now, therefore, conclude this letter with one observation. To the surprise of several other persons, as well as of myself, Mr. Owen, in his "Odontography," states, that when he submitted his theory to the Institute, he was ignorant of the researches of Schwann. How this could possibly be, I am at a loss to divine, *for the report of my paper in the* "Literary Gazette" *contains a full account of these researches*, which I read before the Medical Section, and they are also noticed in the "Athenæum." This account, Mr. Owen shows, satisfactorily, that he has repeatedly seen, long before it was expressly pointed out to him by the gentleman who adopted such a peculiar method of expressing his "aversion to impudence and duplicity."

I think, Mr. Editor, I have succeeded in proving, that in my papers, the teeth were represented as organised, and as being formed by an ossific transformation of the pulp; and that, therefore, Mr. Owen's omitting at first to recognise this fact, in order that he might publish the theory based on these views as a new one of his own, was as unjustifiable as his recent attempts to show that these views were borrowed from him, must necessarily be puerile and fruitless. I have also shown that my observations on the unossified pulp are so similar to his as to have required, at any rate, a notice in the "Comptes Rendus," where he applies all the researches communicated, to the establishment of a new theory of dental formation,—that the identity of the fibres of the ivory with a part of the structure of the pulp was announced by me before it was published by him; and that, with respect to the "deposition" or "arrangement" of the ossific matter in the cells of the pulp, he has but made the same statement that I had previously published. I should be glad to enter still further into detail on this subject, and show the full scope of the papers, drawings, and diagrams, which I submitted to the British Association, and which, of course, are but inadequately reported in the weekly journals of the time; but I feel that this is not the place so to do, nor can I expect, after having trespassed so long on your indulgence, that you should grant me more space.

There is also another proof that Mr. Owen must have been familiar with these researches by Schwann before the publication of his "Memoir," and that is an analogy too close to be accidental between a passage I had read, translated from that author, which is given in the "Literary Gaz.," and a paragraph in Mr. Owen's memoir, contained in the "Comptes Rendus." Of this analogy your readers shall judge by a comparison of the passages. "Against the theory," says Schwann (vide Report of my paper in the "Literary Gazette," No. 1183, p. 598)," that the dental substance is the ossified portion of the pulp, the facility with which the one is separated from the other has been adduced, and he allowed the force of this objection. Nevertheless, it is at any rate weakened by the circumstance, that a portion of the pulp actually remains attached to the dental substance; and by the fact that in half-ossified ribs, for instance, the cartilage can be easily separated from the ossified portion, and it must be remembered that, in both, the separation must be easy in proportion to the difference between the consistence of the pulp and of the dental bone."

"The argument (says Mr. Owen, "Comptes Rendus," for Dec. 16, p. 788) drawn from the slight mechanical connection which exists between the calcified and non-calcified portions of the pulp in the teeth of the mammifera, in favour of the theory of exudation, and, consequently, of the glandular nature of this bulb, might be advanced with almost as much reason, in order to demonstrate that

the primitive cartilage of the sternum secretes or transudes by successive layers the osseous nuclei, which in the embryo can be separated with such facility from the cavities in which they are formed." I think, Sir, that your readers also, after comparing these passages, will be somewhat inclined to suspect that Mr. Owen must have had, at any rate, a *slight* acquaintance with Schwann's researches before the spring of the present year.

An Essay on the Treatment and Cure of Pulmonary Consumption, on principles Natural, Rational, and Successful, &c. By GEORGE BODINGTON. London: Longman and Co., 1840. 8vo. pp. 60.

THE modest and rational preface with which the author introduces to us his pamphlet, on pulmonary consumption, has so far influenced us in his favour, that we shall merely give an outline of his principles, without expending any portion of our critical wrath on his very crude ideas and unsupported assertions.

Mr. Bodington's theory of pulmonary consumption is altogether novel, and far above the range of our limited powers of comprehension.

"The whole structure being viewed as composed of so many parts, the several parts differing from each other in function and structure, the question presents itself, how would each be affected by the presence of a particular morbid affection? as, for instance, a deposition of tuberculous matter; taking, first, the higher order of organisation, the nervous filaments, spread out on the organ thus affected, we should infer that their power would be so affected by the presence of the foreign body, as to be wasted or lost; so that, by diminished power, they could no longer controul and preserve in healthy action the blood-vessels, cellular tissue, and other portions of the common organisation; and as this action of the deposited matter would occur upon the extremities of the nerves, the capillary vessels would be affected by the loss of nervous power, and losing, in consequence, their contractility, or some portion of it, become dilated, swollen, and congested; and then would follow the usual phenomena, commonly called inflammation, terminating in suppuration or ulceration; that is, these vessels, losing the aid of nervous influence, are no longer able perfectly to perform the office of hydraulic tubes, carrying a fluid containing solid particles in solution—the blood; hence, congestion, obstruction, and collection of the solid parts of the blood in these vessels takes place, terminating in abscess, ulceration, gangrene, or re-solution."

The diminished or wasted power of the filamentous extremities of the pulmonary nerves from the presence of tuberculous matter deposited upon or near them is, we confess, a theory for which we were not prepared, and we are equally at a loss to conceive upon what hitherto unobserved facts Mr. Bodington has built up his castle. But facts, with bold and vigorous intellects, are matters of little importance; the theory is "natural" and "rational," and hence the treatment which flows from it must be "successful." The measures, then, which Mr. Bodington assures us have been "uniformly and completely successful in the treatment of several cases of tuberculous consumption," are detailed in the following passages:—

"In order to restore a consumptive patient, it will be necessary especially to attend to the following matters:—We shall find first of all a rapid and weak pulse, ranging from 120 to 140 beats in a minute, clearly indicating a deficient supply of blood, and the heart and arteries irritable in proportion to this deficiency. This condition must be met at once, not by the means termed "antiphlogistic," but with frequent supplies, in moderate quantities, of nourishing diet and wine; a glass of good Sherry or Madeira in the forenoon, with an egg, another glass of wine after dinner, fresh meat for dinner, some nourishing food for supper, such as sago, or boiled milk, according to the taste and digestive powers of the patient. This will be supplying means to rectify the morbid condition of the nutritive functions, and to allay the irritability of the heart and arteries. I have generally succeeded in the course of a few days, or perhaps a week, in reducing the pulse from 130 to 140 down to 90, by means of this diet, and by a systematic use of sedative medicines, and other means. The whole nervous system is unduly excited, or affected some way we know not how to express or understand, from our limited knowledge of it, when under the influence of this disease; and neither can nutrition be effected, or the muscular system recover strength, or the vessels be filled with a due supply of the vital fluid, unless that nervous disorder be allayed and soothed, or rendered more in accordance with a healthy condition. The plan to obtain this object is to give alterative doses of sedatives, and also direct or full ones; the former consist of moderate doses given at intervals throughout the day, with the view of allaying the general nervous excitement. The direct or full dose is given at bed-time, to allay coughing and procure sleep. Aconite, henbane, or the salts of

morphia, may be used; I have preferred generally the hydrochlorate of morphine. A sufficient dose to procure a whole night's repose should be given every night, in addition to the alterative doses above mentioned; the latter may be administered in an almond emulsion, in doses repeated three or four times a-day. Should the medicine produce constitutional effects, paleness, faintness, sickness, giddiness, it must be laid aside for a period, and an antidote will be found in small quantities of weak brandy and water, or wine and water. The sedative medicines should be resumed so soon as these effects are removed.

"I come now to the most important remedial agent, in the cure of consumption, that of the free use of a pure atmosphere; not the impure air of a close room, or even that of the house generally, but the air out of doors, early in the morning, either by riding or walking; the latter when the patients are able, but generally they are unable to continue sufficiently long in the open air on foot, therefore riding or carriage exercise should be employed for several hours daily, with intervals of walking as much as the strength will allow of, gradually increasing the length of the walk until it can be maintained easily several hours every day. The abode of the patient should be in an airy house in the country; if on an eminence the better. The neighbourhood chosen should be dry and high; the soil, generally, of a light loam, a sandy or gravelly bottom; the atmosphere is in such situations comparatively free from fogs and dampness. The patient ought never to be deterred by the state of the weather from exercise in the open air; if wet and rainy, a covered vehecle should be employed, with open windows. The cold is never too severe for the consumptive patient in this climate; the cooler the air which passes into the lungs, the greater will be the benefit the patient will derive. Sharp frosty days in the winter season are most favourable. The application of cold pure air to the interior surface of the lungs is the most powerful sedative that can be applied, and does more to promote the healing and closing of cavities and ulcers of the lungs than any other means that can be employed; for it is by the use of the means which have the power of restoring to a healthy condition the nervous system, interwoven with, and forming a portion of the substance of the lungs, that healthy actions can be induced in the remaining tissues. This, then, is to be aimed at,—a healthy nervous system, which will embrace in its consequences, due sensibility, motive power, nutritive and reparative power,—conditions necessary to resist and overcome the morbid influence arising from the presence of tuberculous matter."

More agreeable or seductive medicaments could not, certainly, be found in any pharmacopœia; fresh morning air to make the patient breathe; good wine to bring down his pulse; a good dinner to make him fat, and an opium pill to make him sleep, are all excellent remedies, if they would only have the desired effects.

Mr. Bodington informs us that in the dry, sharp air of Warwickshire they have marvellous effects, and we are bound to believe him, although we should have desired better evidence than is furnished by the following case:—

"A young man, about nineteen years of age, after having a year before suffered from hæmoptysis severely, and subsequently from slighter attacks of that disease from time to time, became the subject of a very severe hypochondriacal affection, which, in the month of August, 1839, terminated in the development of tubercular consumption, characterised by frequent cough, and expectoration of mucus and pus, or matter of an ashy colour, sinking in water; by nocturnal profuse perspirations, shortness of breathing, emaciation and great debility; pulse ranging from 130 to 150 beats in a minute; respiratory murmur almost imperceptible; percussion over the clavicles gave a dull sound; internal resonance of the voice and cough on the right side; the whole symptoms physical, and natural, clearly demonstrating the existence of ulceration and excavation of a portion of the lungs, constituting the last stage of consumption."

If there be symptoms physical and natural which clearly demonstrate the existence of *ulceration* (?) and excavation of the lungs, or, in other words, constitute the last stage of consumption, then has Mr. Bodington proved his case, and is entitled to national rewards equal, nay, superior, to those conferred on the illustrious Jenner.

Mémoire sur la Pelvimétrie, &c. Memoir on a New Method of Measuring the Pelvis. By Dr. VANHUEVEL, Senior Physician to the Lying-in Hospital, Brussels.

THIS pamphlet contains a brief account of the different modes hitherto employed for ascertaining the diameters of the pelvis, and of a new instrument which has been invented by the author for that purpose. Having entered into a critical examination of the pelvimeters employed by Baudelocque, Kluge, Davis, Chaussier, Boivin, Barovero, Stark, Kurzwich, Koeppe, Adsdrubali, Stein, Aitken, Creve, Simeon, Contonly, Ritgen,

Osiander, and Jumelin; and having enumerated the various objections to which their instruments are liable, Dr. Vanhuevel proceeds to describe his own pelvimeter, which, he assures us, is well calculated to determine with accuracy all the external and internal dimensions of the pelvis. The instrument of Dr. Vanhuevel appears to be a very complicated one; and it will be a difficult matter for the reader to understand any general description of it, without illustrative plates. We shall, however, endeavour to convey an idea of the new pelvimeter, and leave to experience to determine its practical value and facility of application. The principle upon which Dr. Vanhuevel has constructed his instrument, is the well-known geometrical one, that "with two sides and one angle of any triangle, we can determine the third side." The instrument is composed of five pieces; viz.

1. A circular belt which embraces the pelvis, and is furnished at the back part (over the sacrum) with means for supporting,

2, a perpendicular tube, with a ball and socket joint; to which is articulated,

3, an horizontal plate, or arc of a circle, furnished at its anterior extremity with a species of quadrant, which is moveable, and which is connected with,

4, a vaginal rod, of polished steel, 14 inches in length, which plays alongside the quadrant, and serves to determine the diameters of the pelvis, interiorly.

5. An apparatus, somewhat resembling a compass, with a moveable arc of a circle, &c., to determine the pelvic diameters, exteriorly.

To ascertain the internal admeasurements of the pelvis, the surgeon fixes the belt round the sacrum, and having arranged the horizontal rod, according to the diameter which he proposes to seek, forces into the vagina two fingers of the left hand, and keeps them applied on the promontory of the sacrum (we suppose that he wants to ascertain the antero-posterior diameter). With the other hand he introduces the vaginal rod, holding it like a writing-pen, into the vagina, and places the point on the promontory; the rod is now screwed by the side of the quadrant and the point marked. This done, the rod is withdrawn, and again passed into the vagina, behind the pubis, until it rests on the symphysis, while at the same time the quadrant is made to slide backwards on the horizontal branch of the instrument, until it arrives at a point where the vaginal rod can again be attached to it. Thus, the operator obtains two sides and the angle of his triangle, viz., the distance of the sacral promontory, the distance of the symphysis pubis, and the included angle as shown on the graduated arch of the quadrant; from these he deduces, by means of the compass, the third side of the triangle, which is, in fact, the antero-posterior diameter.

We have not had, probably may never have, an opportunity of applying this new instrument; but, according to the testimony of the Belgian committee, which was appointed to examine it, Dr. Vanhuevel's pelvimeter is easy of application, and determines, "with the most rigorous exactness," the different diameters of the pelvic cavity.

CURE OF STRABISMUS.

M. Jules Guerin announces, in a letter recently addressed to the Academy of Sciences, that he has performed Dieffenbach's operation for the cure of strabismus in four cases with success.

I had long ago established (says M. Guerin), and publicly professed, that strabismus depends on retraction of the muscles of the eye, and that its various forms depend on the different degrees of retraction, variously affecting the different muscles which move the eyeball. This is a simple application of my theory of the deformities of the joints in general, which elicited from a distinguished member of the Academy, the quaint, but just remark, that squinting was the club-foot of the eye. In accordance with this theory, I had proposed to extend to the eye the section of its muscles, a practice employed by me in the treatment of deformities in general; the mode of operating which I have adopted, differs slightly from that of Dieffenbach; the results have been advantageous, but not immediately so: in one case only did the eye become quite straight soon after the operation; in the others there was merely an amelioration, and this circumstance appears to me to be a natural consequence of the true origin of squinting.—*Journal des Debats*, July 1, 1840.

THE LANCET.

London, Saturday, July 11, 1840.

THE *Vaccination Extension Bill* has been read a third time in the House of Commons; and the Medical Profession has once more been defeated by the Poor-Law Commissioners. We have no desire to disguise the defeat, congratulating the public, however, on one thing,—that we shall have converted the "Vaccination Extension Bill," into the SMALL-POX PREVENTION ACT. But the direction of the Vaccination of the people has been entrusted to the Poor-Law Commissioners. The Commissioners will draw up the regulations; and they will have the power to annul all contracts which exceed their standard of remuneration, and do not accord with their ideas of propriety. Their power is extended to parishes where they had previously no right to interfere. If the Medical Corporations continue what they are, the Medical Profession will very shortly be trodden still flatter under the feet of this ignorant, irresponsible Commission. Vaccination, it is admitted, is an invaluable boon to the nation. By whom was that boon conferred on the human race? What beneficent profession did he adorn? Among what class of men was the discoverer of Vaccination found? The Medical Profession; the Medical Practitioners of England. How strange, then, that the Legislature, in framing a measure for the Extension of Vaccination, should have confided it to our enemies, and should have refused to entrust any share of its administration to the Medical Profession! Vaccination is to be "extended" by the Poor-Law Commissioners. Medical Practitioners are excluded—we beg pardon, are not excluded from the trouble of performing the operation, but are in every case to be the instruments and servile tools of the Commissioners; they are to "report" to the Commissioners. If the regulations, and the measure for securing the extension and success of Vaccination, require any scientific knowledge, or skill of any kind, what pretensions have the Commissioners to such knowledge or skill? Could the sagacious Secretary of the Commission distinguish a vaccine pustule from a pimple on his unabashed countenance? If it were desirable to conciliate the prejudices of the poor, should the banner of this saving operation have been placed with persons who would be received nowhere with open arms, but in many places with execration,—the founders of the Union Prisons—the water-gruel dietaries—the reduced allowances to the poor, refractory, grey-haired wretches who choose rather to starve in ditches than die in workhouses! If such a measure might have been best carried out by men on whom the mantle, and the beaming benevolence of JENNER had descended, why, in the name of the GOD of MERCY, commit it to the hands of the Poor-Law Commissioners?

Vaccination is enforced and controlled in other countries by Sanatory Boards; but well-informed medical men are at the head of those Boards. The Governments of other countries have no Poor-Law Commission to glorify, at the expense of the Medical Profession. The contempt and degradation of subjection to a Poor-Law Commission was reserved exclusively for the Medical Practitioners of England, because they discovered and propagated the operation that other nations adopted.

Deeply as these events must sink into the memory, we recall them now chiefly to inquire what was the conduct of the Medical Corporations in this crisis, and to demand, whether any thing could demonstrate more decisively than the rejection of the VACCINATION CLAUSES and the VACCINATION SCHEDULE of the Small-Pox Prevention Bill, and the unexampled apathy of the Medical Corporations, the necessity of Medical Reform? The Vaccination Extension Bill struck at the vital interests of the members of the Medical Corporations. It was calculated to interfere in the most annoying way with their private

practice, and to expose them to the humiliations of the contract system. Petitions against the pernicious project, and in favour of a better Bill, were sent from every part of the country. What said, what did, the College of Physicians, the College of Surgeons, and the Apothecaries' Company, of London? NOTHING. It would not affect them. What cared they for medical science? They did not want the trouble of perusing and digesting Vaccination Reports. They could fold their arms philosophically; and look patiently on the sufferings of—twenty thousand surrounding members!

There cannot be the slightest doubt that the triumph of the Poor-Law Commissioners, and the defeat of the Medical Profession, is to be ascribed to the conduct of the Medical Corporations; partly to their wickedness, and partly to their weakness. They live only to expend, and to put the money of the members into their own pockets. In any question of this kind, neither the Government nor the Legislature ever mention the name of the College in Pall Mall; or think for a moment of consulting the Council of *Twenty-one*, who sell diplomas in Lincoln's-Inn Fields; much less the Apothecaries who sell drugs in Blackfriars Hall. All questions of public health are to be referred to the Poor-Law Commissioners. If cholera or any epidemic threatened, the Home-office would not consult the President of the College of Physicians—but that greater Francopulos than he—Mr. EDWIN CHADWICK, the learned Secretary who has devoted himself to the study of dietetics and arithmetic, with sufficient success to make the two surprising discoveries; first, that the "high diet" of gaols, or bread and water, is fatal to an incredible extent; and, secondly, that "two-thirds" and "three times" are convertible terms.

Very recently, at a meeting of the managers of the Charity Workhouse, Edinburgh, "a letter *was read from* Mr. CHADWICK, *Se-*"*cretary to the English Poor-Law Commis-*"*sioners, requesting answers to certain queries* "*relative to the causes of disease and destitu-*"*tion of the poor, and their sanatory condition* "*generally!*" The wolf in sheep's clothing! This is admirable! Mr. CHADWICK inquiring into the causes of disease, and the sanatory condition of the people of Scotland! Mr. CAMBELL, writer of the Signet, very judiciously moved that, "before answering the "queries, application be made *for a copy* "*of the authority on which the inquiries of* "*the English Poor-Law Commissioners* are "extended to Scotland, and the object of "that inquiry." The motion was carried, on a division, by 21 to 8.

Nothing could be more prudent than the preliminary inquiry suggested by Mr. CAMBELL. Though it should turn out that, by some oversight of the Home-office, the Government had commissioned Mr. CHADWICK to inquire into the "causes of diseases," and had thus insulted the Medical Profession of Scotland, and risked the utter failure of the inquiry; this could not have been anticipated by a body of sensible, reasoning Scotchmen.

If any inquiries had been made on the part of Government into the sanatory state of Scotland, the people there would have naturally expected the queries to be drawn up, and addressed to them,—not by Mr. CHADWICK—but by Dr. ALISON, Dr. CRAIGIE, HENRY MARSHALL, Dr. COWAN, Dr. CLELAND, or some of the other highly-talented and respectable physicians of that part of the kingdom.

The Poor-Law Commissioners have succeeded, by the Vaccination Extension Bill, in extending one foot, cloven of course, into all the parishes of England, and now their shadow falls on Scotland. They enter the north under the guise of sanatory inquirers. We wish them all the success they deserve.

The Medical Reformers of Scotland will see in this insulting invasion, which no organisation they, at present, possess can resist, the necessity of renewed efforts to secure the establishment of a representative *National Faculty of Medicine.*

PETITIONS FOR MEDICAL REFORM.

THE number of petitions praying for changes in the laws relating to medicine, presented to the House of Commons, during the present Session, up to the 26th of June, 1840, amounts to 130, and the signatures attached, to 4321; a great increase since our last notice on this subject. Amongst them were petitions

From Manchester, presented by Mr. Mark Philips, M.P.—From Drogheda, by Sir Wm. Somerville.—From St. Asaph, by Sir Stephen Glynne.—From Carrickfergus.—From St. Margaret's and St. John's, Westminster, from Exmouth, and from the Medical Association of Ireland, by Mr. French.—From Mr. Robert Murray and others, and from the County of Monaghan Association, by Mr. Lucas.—From Antrim, by Mr. Gibson Craig.—From Ballycastle.—From the Stirling Medical Association, by Colonel Abercromby.—From Londonderry.—From London, by Mr. Mackinnon.—From Market-hill, by Lord Acheson.—From Holywood, from Ballymena, and from Larne, by Mr. Irving.—From Penrith, by Major Aglionby.—From South Shields, by Mr. Ingham.—From Lurgan, from Comber, from Brentford, and from Broughton-Ferry, by Mr. Warburton.—From Wicklow, by Mr. Grattan.—From Limerick, and from Tiverton, by Lord Palmerston.—From the Clackmannan and Kinross-shire Assoc., by Admiral Adam.—From Renfrew, by Mr. Houston.—From Presteign, and from Reading, by Serg. Talfourd.—From Whitehaven, by Mr. M. Attwood.—From Devonport and Stonehouse, by Sir George Grey.—From Abroath, by Mr. Chalmers.—From Lonsdale, by Mr. Wilson Patten.—From Nottingham.—From Liskeard, by Mr. Charles Buller.—From the Medical Society of Dundee, by Mr. Fox Maule.—From Great Driffield, by Lord Hotham.—From Beverley, by Mr. Hogg.—From St. Marylebone, by Lord Teignmouth.—From St. Martin's-in-the-Fields, by Mr. Leader. (The two last petitions contained, together, the signatures of 139 medical gentlemen.)—From Paisley, by Mr. Hastie.—From Welshpool, by Sir John Edwards.—From Bridgnorth, from Carlisle, from Polperro, from Alythe, from Wexford, from Stowmarket, and from Edinburgh, by Mr. Wakley.—From Lambeth, by Mr. Duncombe.—From Durham, by Lord Dungannon.—From Newmarket, by Mr. Eaton.—From Halifax, by Mr. C. Wood.—From Andover, and from Hexham, by Mr. Blackett.—From Kilkenny, and from Marsham, by Mr. Hume.—From Whitby, by Mr. Chapman.—From Cork, from Kilrush, and from Newcastle-on-Tyne, by Mr. Ord.—From Teignmouth, by Sir Chas. Grey.—From Berehaven, by Mr. Beamish.—From Tavistock, by Mr. Rundle.—From Gateshead, by Mr. Hutt.—From Berwick-on-Tweed, by Mr. R. Hodgson.—From the Eastern Association of Scotland, by Sir H. Parnell.—From Sunderland, and from Kingston-on-Hull, by Mr. Broadley.—From the Kidderminster, Bewdley, and Stourport Medical Book Society, by Sir Thomas Winnington.—From the Glasgow Association, and from the Glasgow Faculty of Medicine, by Mr. Dennistoun.—From the Cork Eastern Society, by Mr. W. O'Brien.—From St. Pancras (182 signatures), and from one of the Northern districts of London, by Mr. Hawes (62 signatures).—From the North of England Association, by Lord Howick.—From the Wexford Association, from the Armagh Association, from Parsonstown, and from Ennis, by Mr. C. O'Brien.—From the North Tipperary Medical Union.—From the Edinburgh College of Surgeons; and from Roscrea.

Those petitions, the members presenting which are not mentioned above, were forgotten to be countersigned with the names of the M.P.'s, previously to handing them to the clerk of the House. The information under that head is therefore not now obtainable. The presentation of these petitions has not been accompanied by any objection to the prayers, in any one instance, on the part of the members to whose care in the House of Commons they have been confided.

THE PROVINCIAL MEDICAL ASSOCIATION IN SHREWSBURY.

To the Editor of THE LANCET.

SIR:—I this day send you the "Salopian Journal," July 1, containing an account of the annual meeting of the "Shropshire-and-North Wales Branch of the Provincial Medical and Surgical Association, held June 25th, at Shrewsbury. The managing parties refused to admit any reporter excepting from the above paper, which is the local organ of a party. I am surprised that such a high-minded and liberal gentleman as Mr. W. Clement, of Shrewsbury, could countenance such a proceeding by his presence. The reporter for the "Shrewsbury News," the best conducted and most talented paper in the county, was not only refused admission to the meeting, but grossly insulted also. Your obedient servant,

NEMO.

Salop, July 2, 1840.

*** As NEMO has not authenticated the above statement, we may hope that it is incorrect, though the parties really could have gained nothing by an increase of the reports of a meeting whose chief characteristic was the exchange of eulogies between the com-

pany,—probably the "great design" of the assembly. Respecting the "parent" association and its "founder," Dr. JEFFREYS thus occupied attention:—

"I believe its increase, its prosperity, and its success is quite unprecedented, in the rise and progress of any such a body of men upon record, and which has mainly been effected by that one individual, whose presence at this meeting was fully expected, and which I am confident would have been the case had not some very pressing and urgent necessity prevented it. The duties of his office are of that nature, and so extensive, that I do not know the person in existence who would, or perhaps could, so efficaciously carry them into effect, and who has handed his name down to posterity in a manner which entitles him to exclaim with the poet,

'Exegi monumentum ære perennius.'

And I may with truth also add,

'Hic et ubique terrarum.'—(Cheers.)"

The Legislature may except Shropshire from any mention in its enactments against quackery. The President, Mr. RICE WYNN, in his address, thus justifies that exemption:

"There is another subject which has attracted my attention, but I confess I have not hitherto found time to consider it fully and fairly in all its bearings,—I allude to quackery. Empiricism, in some of the northern and other counties, is tolerated, it is true, and countenanced to an overwhelming extent, by those who probably on all occasions adopt profession, and not principle. But in this, our immediate neighbourhood, Gentlemen, I have the satisfaction of being able to say, that we know very little comparatively of any serious injury which it inflicts either on the members of the medical profession or on the community, and I feel prepared to account for the cause of our happy exemption. Gentlemen, for a longer period than the last half-century, our profession has had one at its head to whose experience and example it might safely look for guidance—a gentleman distinguished for his devotedness to his profession, as to his success in it—a gentleman whose extraordinary talent in the discrimination of diseases, and ability in the treatment of them, and whose undeviating moral integrity and fidelity of purpose have raised him to a just pre eminence, who, himself incapable of the slightest dereliction of duty, would never allow it, in another, to escape the rebuke of honest and manly indignation. Gentlemen, I need not say that it is to Dr. Darwin we are mainly, if not entirely, indebted for our prosperity and respectability. (Applause.) As practitioners we have, I hope, humbly endeavoured to imitate his bright example, and have shared his success. And to the younger branches of our much envied, though laborious and anxious profession, I would beg to be allowed to say 'Go and do likewise.' (Cheers.)

"In reply to a remark from Dr. J. P. Johnson, as to the expediency of publishing an analysis of patent medicines,

"Dr. HASTINGS said, that the subject of quackery, as well as that of medical reform, had been touched upon at the last general meeting, at Shrewsbury. They had petitioned the Legislature in favour of medical reform, and though some time must elapse before the bill could be brought forward, Mr. Warburton was pledged to it. (Applause.) On the subject of quackery, it had been agreed to, on the suggestion of Dr. Barlow, that the Committee's further steps should be suspended until after the Southampton meeting.

"Mr. BROOKES, of Wenlock, said, that he wished to bring before the Association the treatment which Dr. Johnson and Mr. Keate had last year received from the Salop Infirmary Directors.

"The Society declined entering into any discussion relative to Mr. Keate, Dr. Johnson, and the Salop Infirmary; and it was communicated by Mr. Brookes that Dr. Johnson and Mr. Keate had withdrawn their request for its introduction."

High compliments were paid by the President to a deceased practitioner of the town:—

"In conclusion, I beg to trespass a little longer only on your attention and time, while I offer to you a brief, though very inadequate tribute and testimony to the memory of our most deeply-lamented late associate, townsman, and friend, Mr. Sutton. I was in constant intercourse with him from the very commencement of his earliest career of public usefulness. He was ever distinguished by his simplicity and probity of character, his mildness and suavity of manners, his great and acknowledged practical skill, his entire devotedness to the harassing and arduous duties of his profession, and the alleviation of suffering and distress. In private life he was universally beloved for his amiable and social qualities and disposition. Gentlemen, Mr. Sutton's memory will meet with its enduring reward. (Applause.)"

Dr. HASTINGS, in the course of his address, speaking of the achievements of the Parent Association, said—

"It had drawn attention to the subject of vaccination, and hence had arisen the Bill for preventing indiscriminate inoculation, although he thought it an error to have placed in the hands of the Poor-Law Commissioners what should be done by the National Vaccine Board. (Cheers.) The Association had also established a Benevolent Society, from which the best consequences must flow. Their uniting had enabled them to do what an individual could scarcely have

attempted: it had enabled them to enrich the new volume of their 'Transactions' with the beautiful plates of Mr. Ceely, at the cost of £700,—coloured plates, which illustrated his proof that small-pox and cow-pock were the same,—a fact in medical science obtained by a series of deductive experiments and proofs unsurpassed by any thing since those of Jenner. (Great applause.) It was the funds of the Association which enabled this discovery thus to be made public, and the 'Vis unita fortior,' collecting guineas from many, raised a sum which few individuals could spare."

Dr. JEFFREYS made reference, during the business of the meeting, to

"An article of the Materia Medica, very little known in this country, but one of great and general use in South America. This herb he exhibited at the meeting of the Association in Liverpool, July 24, 1839. It is a styptic of great power and efficacy, and is called 'Yerba Soldado,' or the Soldier's Herb, from its astringent property being accidentally discovered by stopping the bleeding vessel of a wounded soldier in the field of battle. It was also called *Matico*. Dr. Kenrick, of Warrington, applied a little of it to an artery, which was divided by a gentleman who attempted suicide by cutting his throat, and it stopped the hæmorrhage instantly. An order for an ample supply had, by an accident, not yet arrived in this country, but it was expected by every vessel from Valparaiso which may come to Liverpool: and when it was received, he would take care that the Association should know where such supplies may be obtained. It is no less efficacious as a styptic for internal, as for external hæmorrhage, given in form of decoction and infusion."

The meeting was commenced, as it would seem, in private, which might account for the exclusion of any reporter but one who could be under the controul of the managers. The "Salopian Journal" says it was attended by

"Between forty and fifty members, who assembled on the invitation of Rice Wynne, Esq., President of the Branch, at the College, his residence. He had invited several of the gentlemen of the town and neighbourhood to meet them at a splendid *déjeuné à la fourchette*, which was set out in the temporary room formerly belonging to the late Mr. Cressett Pelham, M.P. This room was erected in the garden, and the entertainment did the amplest credit to the proverbial hospitality of Mr. Wynne."

Shortly after the toast of the "Visitors and Clergy" had been drunk,

"The visitors retired, and it was agreed that the business of the meeting might be more conveniently transacted without the members removing to the Lion, as named in the public advertisements. We may add that, during the transaction of business, there was a total cessation from the social pleasures of the table.

"Shortly after six o'clock, about thirty-two gentlemen sat down to dinner at the Lion. The chair was taken by Dr. Parker, supported by Dr. Hastings and Dr. Jeffreys, and Mr. W. Clement performed the duties of Vice-Chairman."

It may be mentioned, for the information of the governing bodies in all the Colleges of Physicians and Surgeons, and all the Apothecaries' Halls, in the United Kingdom, that "the Queen" and all the loyal toasts were drunk; that "the Association" was drunk; that "Dr. Jeffreys" was drunk; that "Dr. Hastings" was drunk; that "Dr. Darwin," "Dr. Parker," "Mr. W. Clement," "Mr. Wynne," "the Guests," "the Secretaries," were all "drunk," twice over, at breakfast and at dinner, with cheers and heartiness; but that the poor colleges and corporations, those institutions at one time invariably glorified by bacchanalian cheers of nine-times-nine duration, were, not one of them, morning or evening, drunk the least in the world.

Dr. PARKER left the chair at nine o'clock; but the enjoyment of the evening was continued for a few hours longer.

MEETING OF MEDICAL MEN AT NEWTON.

SMALL-POX AND VACCINATION.

MEMBERS of the medical profession from Liverpool, Manchester, Wigan, Leigh, Warrington, and other towns in Lancashire, attended a meeting of the Newton Branch of the Provincial Medical and Surgical Association, at the Leigh Arms Hotel, Newton-in-the-Willows, on Thursday fortnight. Dr. JEFFREYS, of Liverpool, presided. The report of the Council, read by the secretary, Mr. HARDY, adverted to the urgent demands, which had every where prevailed, for medical reform; and congratulated the members on the unanimous determination which prevailed in the profession, to induce the Legislature to grant to them just laws and equal privileges. Reference was also made to the Bills before the House of Commons for "extending vaccination" and abating small-pox; but as the statements contained in the report in the "Manchester Courier" (June 25), would argue but a very poor un-

derstanding of the chief question relating to those measures, we take it for granted that an incorrect abstract of this part of the Secretary's document has been inserted, especially since "the report was unanimously adopted, and ordered to be printed." The newspaper says,

"The council felt called upon to notice the bills now before Parliament relative to vaccination, which may, in some measure, be regarded as the first fruits of the endeavours of the provincial association to determine the true value of this grand agent for the prevention of small-pox. The object of both bills is the same, viz., to extend the practice of vaccination. Both provide that this shall be done by legally qualified practitioners only; but there is this difference; whilst one (that of Mr. Wakley) determines the amount of remuneration to be received for the vaccination of every pauper, the other leaves the point open to the same auction-like bargaining which characterises the mode in which medical relief is at present provided for the poor of this country, and is therefore highly objectionable. There is also another difference, which is not less important. Whilst Lord Ellenborough's bill indirectly sanctions the practice of inoculation, the other positively forbids it altogether, and consigns to prison every practitioner who may be guilty of it. Having pointed out the main features of both bills, it may be desirable (the council go on to observe) to consider how far it is incumbent upon this association to adopt any proceedings in reference to them. They are of opinion that all the objects contemplated would be best accomplished by one bill, and that both those before Parliament are in their present shape equally objectionable. Any measure which sanctions the practice of inoculation, either directly or indirectly, they conceive to be dangerous to the public health; and any law which consigns a medical man to prison for an act which *circumstances* may require him to perform, is too arbitrary and despotic for the enlightened age in which we live."

What are those "circumstances?" If this report *be* correct, let Mr. Hardy take the trouble to announce them. He seems to have seized the opinion that to enforce *vaccination* was despotic, and with an incomprehensible notion respecting the "fitness of things," to have tacked it to the proposal to prevent the instilment of *a poison*.

An extempore address was subsequently delivered by Dr. KENDRICK on small-pox and vaccination. Amongst other statements the worthy physician made the following:—

"It had been the fashion of late to suppose that it was necessary to *re-vaccinate at certain periods*, and seven years had been fixed upon as the proper interval. In order to show the fallacy of such an opinion, he would state the result of his own experience. It was as follows:—At 32 years from the period of vaccination one patient had had modified small-pox,

Years.	Patients.	Years.	Patients.
At 21	5	At 3	36
20	5	2	26
19	7	1	5
18	19	Months.	
17	20	10	1
16	14	8	1
15	21	7	2
14	19	6	1
13	7	5	1
12	17	4	1
11	19	2	1
10	25	Weeks.	
9	24	10	1
8	22	7	1
7	16	6	2
6	28	5	2
5	40	3	1
4	42		

Now, allowing it to be true that re-vaccination was necessary, at what period would they re-vaccinate? He knew of none. This was certainly the most important part of what he had to say. The real fact he wished to point out was that in some cases, probably one in fifty, vaccination acted only as a partial preservative against variolous infection. It was stated by Rouchoux that at Marseilles the relative number was as great as one in fifteen; but Mr. Cross, of Norwich, had come to the same conclusion as himself, that not more than one person in fifty was liable to modified small-pox after vaccination.

"Wherever vaccination acts only as a partial preservative, variolous disease assumes what may with propriety be termed a modified form; and whenever it appears in that form, the symptoms essentially differ from those attending either the inoculated or casual small-pox. What are those differences? They are very marked. In the first place, in severe cases of modified small-pox, whether confluent or distinct, they are attended with distressing delirium during the eruptive fever; which delirium, in some instances, has continued to the crisis. Convulsions had not been observed by him. It was of importance to remark, that where the pustulous eruption is attended with extensive erythema, no delirium takes place. In all cases of modified small-pox the disease is shortened in its duration; no secondary fever takes place, and it very seldom happened that any disfiguration followed. After quoting cases in proof of these facts, the doctor proceeded to observe, that the fact that in cases of modified small-pox no secondary fever took place, was the true solu-

tion of its not proving fatal, as in 99 cases out of 100 of small-pox the patients were cut off by secondary fever. It might be asked, was modified small-pox never fatal? To which he replied, that out of the 445 cases previously quoted, and which occurred during the years 1818, 19, 20, 21, 22, 23, 24, 25, and 26, none proved fatal. Since then he had kept no record of the number of cases occurring within the sphere of his practice; but two fatal cases of modified small-pox had occurred, in both of which the patients sank under severe purpura hæmorrhagica. He had previously supposed that death could not take place except from suffocation; but, although he had seen many cases in which pustules had existed to a considerable extent in the fauces, suffocation had not followed. This he attributed to the rapidity of the absorption.

"The next point was, that there was no reason to believe that the vaccine lymph had lost any of its protective power by transmission, but that wheresoever its insertion was followed by the formation of a well-formed vesicle, surrounded by an *areola*, it afforded every protection of which it was capable.

"Modified small-pox was liable to excite all those constitutional disturbances which follow casual, or, as it was commonly called, natural small-pox. This had happened in 86 of the 434 cases which had come under his notice. It appeared more than probable, that the imperfect security afforded by vaccination, arose from some constitutional peculiarity hitherto unexplained. The generally received opinions concerning the causes of failure were four. 1st. Imperfect vaccination. 2nd. Deterioration of the vaccine lymph by transmission. 3rd. Temporary nature of the protection. 4th. Too great abstraction of lymph from a well-formed pustule. The second and third causes had been already treated of. With respect to the first, as far as his experience extended, imperfect vaccination was always followed by regular small-pox, although he was sorry to say the contrary opinion had been sanctioned by one for whom he felt a high regard. With respect to the fourth, although he should be the last to encourage too great an abstraction of lymph, he had never found that, provided it was evident from the symptoms that any vaccine lymph had passed into the system, that the security was diminished. The next point was, that nothing less than the extermination of small-pox could remedy the evil complained of, and that all were in duty bound to use every practicable means to bring about so desirable an end. There was a time when small-pox did not exist, and he hoped a time would come, when it would be no longer known."

The case referred to in the following sentence must have made a pretty strong impression on the mind of the surgeon:—

"Dr. Marshall gave a detailed account of an operation performed by him, about thirty-seven years ago, in a case of affection of the bladder."

Before the meeting separated, its thanks to the chairman were carried by acclamation.

"Shortly afterwards, about forty of the members sat down to an elegant dinner, served up with great profusion by the new landlady of the Leigh Arms. Mr. M'Culloch presided, and Mr. Goodlad occupied the vice-chair. On the removal of the cloth, the various loyal and constitutional toasts were given, and drank with enthusiasm, and, the day being the anniversary of the battle of Waterloo, the health of the Duke of Wellington, as the hero of a hundred fights—not as a statesman—was not forgotten. The healths of the various officers were successively given; and nothing was omitted tending to promote the gratification or good feeling which appeared so cordially to exist amongst all present."

UNIVERSITY OF LONDON;

BACHELOR OF MEDICINE—FIRST EXAMINATION, 1840:

Forty-eight Candidates presented themselves for this Examination,

Monday, July 6th, Morning, 10 *to* 1.

ANATOMY AND PHYSIOLOGY.

Examiners, Mr. KIERNAN *and* Dr. SHARPEY.

1. DESCRIBE briefly the form and structure of the sacrum, its articulations with the neighbouring bones, and its mode of ossification; name the muscles to which it gives attachment.

2. What motions take place in the following joints, and to what classes of joints do they respectively belong? 1st. The articulation of the bones of the forearm with those of the carpus. 2nd. Of the carpal bones with each other, and with the metacarpal bones. 3rd. Of the metacarpal bones with the phalanges. 4th. Of the phalanges with each other. Enumerate the muscles of the forearm and hand, classifying them according to their actions; and mention the order in which the tendons are placed round the wrist-joint, commencing with the flexor carpi ulnaris, and proceeding outwards.

3. Describe the parts seen on the surface of a transverse section of the thigh about its middle, and their relative positions. Mention the names of the vessels which usually require ligature, and the situations in which they are to be found.

4. The integuments being removed for a hand-breadth above, and to the same extent below the occipital protuberance, and as far forward as the anterior edge of the mastoid process; describe the muscles,

blood-vessels and nerves thus brought into view, with their relative position.

5. Describe the dissection required to show the course and distribution of the gluteal and ischiatic arteries, commencing at the integuments of the gluteal region, mentioning the parts cut through or exposed in the order they are met with, and describing the arteries and their branches with their relations to the adjacent parts.

6. State the leading facts which serve as proofs of the circulation of the blood.

Monday, July 6th, Afternoon, 3 to 6.

1. Describe the parts met with in the successive stages of the dissection of the inguinal region. Describe the inguinal canal, and its contents in the male and in the female, and the alterations occasioned in the canal by hernia. How is the cremaster formed, how are its fibres arranged, and what varieties does it present? What is contained in the canal prior to the descent of the testis; at what period does the descent take place, and what changes take place in the canal after the descent? How is congenital hernia formed, and in what respects does it differ from the ordinary form of external inguinal hernia?

2. Describe the parts met with, in dissecting from the integuments to the median plane of the tongue, that space which is bounded above by the lower jaw, below by the os hyoides, anteriorly by the median line, and posteriorly by the pharynx.

3. Describe the structure of a lymphatic gland. What has been ascertained as to the mode of origin of the lymphatic vessels?

4. What is seen on the surface of a vertical section carried through the cerebrum, cerebellum, tuber annulare and medulla oblongata in the median plane?

5. Name the nerves distributed in whole or in part to the following muscles, viz., sterno-thyroideus, deltoides, pectoralis major, serratus magnus, trapezius, coracobrachialis, gluteus maximus.

6. Describe the course and distribution of the laryngeal nerves, and state what is ascertained respecting their functions.

7. Mention the differences in structure between the circulating organs of the fœtus (at birth) and those of the adult.

Tuesday, July 7th, Morning, 10 to 1.

CHEMISTRY.

Examiner, Professor DANIELL.

BY EXPERIMENT.

The solutions of two salts will be placed before you, marked A and B, with appropriate tests, labelled: apply the tests, describe the phenomena which take place, and explain the constitution of each salt.

1. In what does the boiling of a liquid essentially consist? and how is the temperature of a liquid connected with its boiling?

2. According to Dulong and Petit the boiling point of mercury is 360° centigrade: to what degree of Fahrenheit's scale does this correspond?

3. Take two similar thermometers, and, on a calm, clear night, place one of them on wool fully exposed to the aspect of the sky; place the other similarly on wool under the cover of a tree; what would you expect them to indicate after the lapse of a short time? and to what cause would you ascribe the effect?

4. I have measured 21.5 cubic inches of a gas standing over mercury; the level of the mercury within the jar is half an inch higher than without; the height of the barometer is 29.74 inches, and the temperature 52° Fahr., what is the correct volume at standard pressure and temperature?

5. State and exemplify the laws of definite and multiple proportions.

6. What is meant by the statement that 40 parts by weight of potassium are equivalent to 8 parts of oxygen and 16 of sulphur; and how would you illustrate the meaning by the constitution of sulphate of potassa?

7. Describe the constitution, mode of production, and principal properties of ammonia.

8. Describe the constitution of nitrate of ammonia, and explain by symbols, and by a diagram relating to volumes, the changes which it undergoes, upon being exposed to a temperature a little above its melting point.

Tuesday, July 7, Afternoon, 3 to 6.

MATERIA MEDICA and PHARMACY.

Examiner, Mr. PEREIRA.

1. Describe the usual mode of procuring sulphuric ether. Explain the chemical changes which take place during the process. State the atomic composition of alcohol and ether.

2. Give a botanical description of *Digitalis purpurea*, especially describing the leaves and flowers. State to what class and order, in the sexual system of Linnæus, this plant belongs, and what is its natural order. Describe its effects and uses; point out the cautions to be exercised in the employment of it, and mention the dose of its powder, its infusion, and its tincture.

3. State for what particular cases the most frequently employed cathartics are respectively adapted or unsuited, and why. Mention what are the appropriate purgatives for febrile complaints, alvine obstruction with great irritability of stomach, inflammation of the urinary organs, and sluggishness of the colon; and what purgatives are improper in diseases of the rectum, in uterine irritation, and after operations about the pelvis and abdomen.

4. With what substances is scammony usually adulterated, and how is their presence to be demonstrated? What are the physical and chemical characters by which the goodness of *Elaterium* is ascertained? How would you proceed to detect the presence of tartaric acid in suspected powder of citric acid? By what physical characters are the leaves of *Cynanchum Argel* found in Alexandrian senna, distinguished from the *Cassia* leaflets?

5. Describe the effects, both of medicinal and poisonous doses of opium; and point out the peculiarities of its narcotic operation. Mention the appropriate remedies in poisoning by this substance.

6. What are the best disinfecting processes for the sick chamber, and for uninhabited buildings?

STRUCTURAL BOTANY AND VEGETABLE PHYSIOLOGY.

Examiner, Professor HENSLOW.

1. Explain the terms axil, bract, tendril.
2. Define an arillus, and mention examples where it occurs.
3. Describe the cellular tissue, and its various modifications.
4. Describe the specimens marked 1, 2, 3.
5. Give sketches of the principal forms of Vernations.
6. Explain the structure and functions of Pollen.
7. What is meant by vegetable irritability and sleep; and what are the external stimuli affecting these properties?

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, June 23, 1840.

Sir B. C. BRODIE, Bart., President.

Case of Fractured Spine Treated by Extension. By H. CROWFOOT, *Esq., Beccles.*

THE patient was a coachman, and received an injury in driving a carriage under a gateway obstructed by a beam, by which he was bent almost double. The spinous processes of the ninth and tenth dorsal vertebræ were found on examination to be separated from each other much beyond their usual distance; the ninth being, apparently, forced forwards, whilst the tenth projected backwards. The bladder was paralysed; and there was a total loss of the power of voluntary motion in the lower limbs, with an equally complete abrogation of sensibility. Unwilling to leave the patient to his fate without some decided attempt to relieve him, the author resorted to the use of extension by means of pulleys, by which the deformity of the spinal column was evidently diminished, with the restoration, at the same time, of a small degree of sensibility; voluntary power, however, was not at all restored. The most active antiphlogistic measures were afterwards pursued for a very considerable time, with the result that, in about eleven months from the accident, the patient was restored to the power of renewing his duty as a coachman. The author considers the final recovery of the patient to have been chiefly due to the steadily-continued efforts to prevent, or subdue inflammatory action in the injured parts.

Sir B. BRODIE remarked, that in the paper which he had communicated to the Society on injuries of the spine, and which was published in the "Transactions," two cases of fractured spine were referred to, in which the same plan of treatment was adopted as in Mr. Crowfoot's case.

The first of these cases came originally under the care of Mr. Arden, of Epsom, and was subsequently conveyed to St. George's Hospital. The patient had received an injury by which some of the lumbar vertebræ were fractured and displaced. Extension was made by force applied to the pelvis and thorax, under which proceeding Mr. Arden said, that the bones resumed their natural position with an audible snap. The lower limbs, however, remained paralysed, although the patient lived for a long period afterwards. The second case was under his own (Sir B. Brodie's) care in St. George's Hospital. In this instance the patient was a boy, who had fractured and displaced some of the bones of the spine. Extension was employed, and the displaced bones reduced to nearly their natural position. No bad results followed the proceeding; the boy lived a long time in the hospital, he was afterwards removed, and he (Sir B. Brodie) lost sight of him. In this case, as in the former one, the use of the lower limbs was not recovered.

Mr. BRANSBY COOPER thought it was doubtful from the history of the cases referred to, and also of that read to the Society, whether extension could be safely or judiciously applied in similar cases. It was always impossible to ascertain to what extent injury might be inflicted by the employment of extension, for the chord might become lacerated, and the case rendered more unfavourable. In the most favourable cases, extension, if thought advisable, should be very cautiously applied. He attributed the great benefit, which resulted from treatment in Mr. Arden's case, to the antiphlogistic measures which had been resorted to.

Sir B. BRODIE observed, that in regard to the necessity for great caution in the employment of extension in fractures of the spine, it should be borne in mind, that the removal of the pressure did not remove the chief part of the mischief, which was inflicted upon the chord; and the more the bones were pulled about, the more likely was that disorganising process in the spinal marrow to go on.

Mr. Stafford had read a case, a few years since, to the Society, in which a man fractured the eleventh dorsal vertebra, and yet was free from paralysis. The patient had fallen from a great height; the fractured portions were so widely separated, that the fingers could be placed in the hollow so formed. The spinous processes were perfect; several surgeons saw the case, and were satisfied of its real nature; the patient was laid upon his back, and subjected to antiphlogistic treatment. A depression still remained at the fractured part, but the patient otherwise quite recovered.

Mr. PAGET had seen a preparation, a few days since, illustrative of a case of fracture of the spinal column, in which the patient recovered. All he knew of the history of the case, consisted in the fact of the patient, a woman, having fallen from some height upon her nates. She was found to have paralysis of the lower extremities; she lived for a long period. After death the tenth dorsal vertebra was found to have been fractured, and displaced to such an extent that it reduced the spinal canal to half its usual size. Union had taken place, and, with the exception of the paralysis of the lower extremities, the patient had perfectly recovered.

Case of Tumour in the Pelvis, Impeding Parturition. By J. C. W. LEVER, *Esq., communicated by Dr. Merriman.*

The patient, who was the subject of the affection described, had been many hours in labour when Mr. Lever was called to see her. Finding a tumour, as large as a fœtal head, occupying the middle of the pelvic cavity, and obstructing the progress of labour, the author punctured it with a lancet, and gave exit to upwards of a pint of an oily fluid, when the tumour collapsed; the pains continuing, the head of the child rapidly advanced, and birth was affected in two hours from the operation.

The case here related bears a close resemblance to some of those described by Dr. Merriman in the 10th volume of the Society's "Transactions."

Memoir on some Principles of Pathology in the Nervous System. By MARSHALL HALL, *M.D., F.R.S.*

This was the third of a series of memoirs on some principles of pathology in the nervous system, read before the Society. The first of these memoirs was "on the condition of the irritability of the muscular fibre in paralytic limbs;" the second "on the reflex and retrograde actions of the spinal marrow;" and the present treats of "the influence of volition, of emotion, and of the vis nervosa;" and completes the account of the sources of motion as exhibited in the various diseases of the nervous system. An attack of complete hemiplegia, leaves the limbs of the paralysed side, but especially the arm, deprived of voluntary motion; the patient generally carrying that arm, the rest of his days, in a sling. Immoveable by volition, however, the arm is moved energetically on any occasion of emotionary hurry, alarm, surprise, &c. The source, the seat of volition, is cut off from the affected limb; that of emotion still remains in connection with it; these are, therefore, different, and it seems probable that that of the former is placed higher up than that of the latter, the two occupying the cerebrum and the medulla oblongata respectively. But besides the occasional effects of emotion, there is a constant influence of the *vis nervosa*, seated in the spinal marrow, and inducing contraction of the fingers and of the wrist of the hemiplegic hand and arm. These are facts of clinical observation, valuable as facts, and in their relation both to physiology and pathology. Who does not now see the distinction, for example, between volition, emotion, and the vis nervosa, as sources of muscular motion? In fact, an attack of decided hemiplegia severs and dissects, as it were, the cerebral from the true spinal system. Tetanus affects, at first, the true spinal, to the exclusion of the cerebral. The marked and deleterious influence of emotion, in the latter case, is emphatically set forth, the author affirming that the intrusion of visitors, &c., is frequently the cause of a fatal termination, which might possibly have been averted by seclusion in a still, dark room, free from the visits of strangers. The author proceeds to speak of the effects of emotion in cases of stammering, of chorea, paralysis agitans, &c., and goes so far as to suggest that the two former might scarcely manifest themselves if all emotion were excluded. They subsided, during quiet sleep, to return on the occurrence of dreams or waking. How are these facts to be explained, except on the principle of the recovered and re-admitted influence of volition? It is well known, on the other hand, how emotion aggravates these diseases at other times. An interesting case is briefly detailed, and illustrated by a drawing of a patient who could seize and hold a pen by a strong and sustained effort of volition; but when this effort subsided, the pen was drawn into various positions by the involuntary and irregular agency of the vis nervosa.

The paper concludes by a brief reference to the influence of shock on the nervous system. In the first attack of hemiplegia, the paralytic muscles manifest less irritability than those of the opposite side; a state, the very contrary of which is observed in the sequel; immediately after the accident by which the spinal marrow is injured, the reflex actions, afterwards observed, are absent. In some experiments on the circulation of the blood in frogs, it was found if, when the brain and spinal marrow were

entirely removed, the circulation still continuing in the web, one foot was suddenly crushed, the circulation instantly ceased in the other. A blow on the tibia produces immediate, though momentary, paralysis of the leg. These are examples of the influence of shock, and obviously afford subjects for investigation.

Mr. Bransby Cooper spoke at some length, for the purpose of showing that the nervous system could not, in his opinion, be divided into distinct parts, each of which had a distinct function. He contended that the integrity of the entire nervous structure was essential to the proper manifestation of the functions of any particular part. He did not understand in what way Dr. Hall used the term *vis nervosa*. In his (Mr. Cooper's) opinion, it could express only the powers of the whole nervous system, and not be limited to any particular nerve or set of nerves.

Dr. Hall replied, that his object in the memoir before the Society was not to treat again of subjects which had long been before the medical public, but to show the application of his former investigations to practice. His two former memoirs had treated of the irritability of the muscular fibre in paralytic limbs, and of the reflex action in limbs paralysed to sensation and volition; his present object was to point out the *occasional* influence of *emotion*, and the *constant* influence of the vis nervosa (which term he used in the sense of Haller, and had defined it accordingly,) in cases of hemiplegia, and in cases of undue excito-motory influence, as tetanus, chorea, &c. It was unnecessary to repeat the already too often given proof of the truth of his system. It was necessary to go on, and to show its useful applications in pathology and therapeutics. He would venture to say, that in future, no cases of nervous disease could be taken or described without a reference, tacit or expressed, to his system; and it was impossible to go the rounds of a hospital without seeing its application. In hemiplegia we saw the paralytic hand and arm moved by emotion, as surprise, by certain acts of respiration, as yawning, sneezing, and by stretching; and we see the fingers drawn into the palm of the paralytic hand by the direct action of the vis nervosa; in paraplegia, arising from disease situated above the lumbar vertebræ, we saw the influence of the reflex function, on tickling or pinching the paralytic foot, &c. These facts afforded sources of diagnosis, as he had pointed out in his former and present memoirs.

He reserved for a fourth memoir to treat of the plan of observation of diseases of the nervous system.

Mr. Le Gros Clark thought that nothing was more obvious than the just distinctions which had been established in regard to the functions of the nervous system, and their applications to practice.

Sir B. Brodie observed, that Dr. Hall's use of the term vis nervosa in the sense of Haller was quite distinct, and he agreed with him and Mr. Clark that the nervous system was obviously divided into distinct parts. Legallois had proved, that respiration, for example, depended upon that part of the medulla oblongata, which gave origin to the eighth pair of nerves. He sliced away the superior portions of the brain successively; respiration immediately and uniformly stopped when this point of that system was removed.

Dr. Hall observed, that in insects each ganglion seemed to involve an analogue of the medulla oblongata, as far as respiration was concerned. This he had ascertained by experiments on the dragon-fly last autumn, experiments which he proposed to repeat during the summer.

CHARING-CROSS HOSPITAL.

MERCURIAL TREMBLING.

Francesco Boeri, aged 55, an Italian looking-glass maker, of short stature and dark complexion, was admitted under the care of Dr. Chowne. He stated that he had worked at the trade in this country 28 years. He had been a muleteer in his own country. During the first five years of his employment in the manufacture of looking-glasses he had enjoyed good health, but at the expiration of that time he began to tremble.

He consequently left his work for two months, and then became as strong as ever. He returned again to his employment, and kept strong for about six years, and then the trembling came on again. In each attack he trembled a good deal; the hands were always the parts affected, the right hand the most so; and this was more employed than the left in his work. In each attack he could walk well; his knees trembled slightly, but not to signify.

He could keep his hands still when he rested upon any thing firm; but the attempt to take any thing in his hands increased the trembling. He was excessively nervous, and if any one even looked at him he shook the more. Anxiety always increased the shaking. The tongue was affected, and the difficulty of speaking was augmented by similar causes. The head sometimes shook a little.

During the whole period, the sense of touch was unimpaired; the vision seemed rather gloomy, or obscure. He sometimes felt as though he were drunk; his thoughts used to wander. He left his work for about two months, and again became well.

The present attack, the third, has been like the former attacks in all essential particulars. The legs, however, have been more affected than they were before; but still not so much so as the hands. He walks sometimes as if he were tipsy, and has often been taken for a drunken man in the streets. The eyes have been less gloomy, the sight less obscure. The tongue is more affected in this attack than it has been in any of the previous ones. Sleep generally good; the memory has been much impaired, more than at present.

During the present attack he has frequently felt as though there would be alvine action, but before he could reach the water-closet was seized with sudden loss of power, and dropped to the ground; the bowels at the same moment acting involuntarily. The falling down was generally, but not always, preceded by the sensation that the bowels would act. He has sometimes fallen and got up again, without the bowels acting; but, generally, it has been otherwise.

If he were not found and assisted to get up, an hour or even two hours would elapse, before he could sufficiently recover himself to rise. Prior to coming into the hospital he fell two or three times a-week, and had the involuntary action of the bowels.

He thinks he should not have been so bad this time if he could have left his work as he did before; but he had had domestic troubles, particularly the loss of several very near relatives, and this he thinks might have also aggravated the disease. Although he has never been salivated, his teeth have been often loose; three of his teeth in front are much decayed: his parents had good teeth, and he does not think that the defect in his own is hereditary.

Upon his admission into the hospital, the general functions were proceeding naturally; the intellects were pretty clear, but subject to great embarrassment from hurry, fright, anxiety, or other excitement. Vision clear; appetite natural. No febrile symptoms or pain; pulse regular, soft, full, and 84. Skin natural; sleep pretty good, tranquil, and refreshing; feels generally weak, in the legs especially. The head, the hands, and the legs trembled; the head least, the hands most. The weakness in the knees very great; the tongue trembling; the speech slow and tremulous, even in the absence of excitement. In every part, the trembling and agitation were increased by hurry and anxiety.

During the first few days he had a tonic steel medicine; but as this appeared to induce constipation, it was discontinued, and a stimulant, aromatic mixture with rhubarb, substituted. A strong stimulating liniment was ordered to be rubbed over the limbs and vertebral column. Under this treatment he considered himself, and appeared to be improving for about three weeks. His diet was nutritious.

At this time tonics were resumed, without the least benefit. Without any obvious cause, a greater degree of debility supervened, the shaking increased, and the whole aspect of the disease became unfavourable. He gradually sunk into a state of greater and greater weakness, for which cordials and wine were administered without effect. This change was not accompanied by any specific change in any organ or function, but appeared to consist of a general failing of the vital powers. The intellect appeared to be clear, and, though weakened, not affected by aberration, until the accession of actual coma, soon after which the patient died.

After-Death Appearances.

The body was examined twenty-four hours after death. The thoracic and abdominal viscera presented nothing unusual. The membranes of the brain were in the natural state. The cerebrum and cerebellum appeared to be healthy throughout, except that to the touch, and when cut into, they seemed to be rather more firm and dense than usual. The colour was not quite so white as natural, and approached towards a shade of dullish yellow. The spinal cord presented no appreciable change. The ventricles were natural, and contained the usual quantity of fluid. The vascular system of the brain healthy.

Dr. Chowne, in lecturing on this case, observed, that as the patient, on his admission into the hospital, was apparently free from organic disease; as the natural functions were, for the most part, proceeding regularly; as the whole of the symptoms seemed to be the result of the employment he had followed; and, as on previous occasions, removal from that employment had been followed by removal of the disease; he had, after finding that the tonic he had prescribed was not without its ill effect upon the bowels, very willingly discontinued it, and prescribed only such medicine as was necessary to ensure regular action of the bowels. The appetite continued good, a sufficient quantity of nutritious food was taken and well-digested, and the sleep was sound and tranquil. When, however, there did not appear to be the desired improvement in the trembling, tonics were again had recourse to, but without effect, and the patient gradually sunk.

The result of the post-mortem examination was satisfactory, inasmuch as it showed that there was not any hidden organic disease of either the thoracic or the abdominal viscera; and that what departure from the healthy state, existed, was in that organ in which the nervous affection might be well believed to have originated; he remarked, however, that he spoke with a degree of

hesitation as to actual change in the structure of the brain. He would venture only to say, that there appeared to be, so far as the sight and touch could distinguish, a slight increase of the hardness and a slight change of colour; but he reminded the students that these were somewhat difficult distinctions, when the departure from the natural appearances was but slight. He also reminded them that such appearances were not necessarily the consequence of disease.

With regard to the diagnosis, the agitation and trembling, under which the patient laboured, resembled, to a certain extent, the agitation of delirium tremens, and the tremulous motions of the hands of persons debilitated by drinking, in whom the disease had not extended so far as to produce delirium with the usual febrile excitement. The diagnosis, however, was not difficult. In tremors from mercury, there was, almost always, entire absence of the spectral and other illusions of the senses, as of hearing and of the touch. There was also, generally, the absence of the profuse clammy sweats, of the restless disposition to get out of bed and to ramble about; of the constant watching; of the peculiar restless expression of the eyes; of the rapid, voluble, noisy talking; and of the mental aberration.

It might be distinguished, also, from the trembling produced by excessive drinking, where delirium tremens had not yet supervened, although, here, the dissimilarity was not so great. The history formed an obvious indication, and the person affected by mercury was so generally aware of the disease, that he could seldom fail attributing it to its right cause. Tremulousness from drinking, however, was generally, in its early stages, temporary, occurred most commonly in the morning, and was frequently removed as soon as a fresh supply of stimulus had been swallowed; the tremulousness from mercury, on the contrary, was not an affair of a short period; was not especially worse in the morning; and was not removed, or even diminished, by a glass of spirits.

The disease bore a stronger resemblance to paralysis agitans than to the tremor which is the result of drinking, either in its mild or severe form; and without some circumspection on the part of the practitioner might be easily mistaken for it. Although, however, the general resemblances were strong, there were specific peculiarities by which they might be distinguished. In this, as in trembling from spirituous liquors, the history was a prominent feature. The attack was generally gradual, and, for the most part, unconnected with symptoms of ordinary cold, while paralysis agitans was frequently preceded by exposure to cold or to damp, and would come on somewhat suddenly; the symptoms of cold passing away, and the paralysis remaining. There would be a further diagnostic guide in the character of the trembling, and in the circumstances under which it became augmented or diminished. In paralysis agitans, supposing the lower extremity, for example, to be affected, it would be seen to tremble, at all times, as well when the limb rested on the ground, or was otherwise supported, as when it was not supported. The trembling from mercury, on the contrary, was increased excessively by nervous emotion, or by muscular effort; the patient, on taking anything in his hand, found the trembling the greater as he increased the effort, and it approached nearer the object of which he wished to obtain possession. The trembling was said to have been so strong in some, that they had not been able to convey their food to their mouths, and they had fed like quadrupeds. In some cases, as in that under consideration, the head was also affected with the trembling motion. In ordinary paralysis agitans, it often happened that the greater the effort required to perform any movement, the more steadily it was performed, and persons who could not walk without extreme agitation and irregularity, were able to run with tolerable steadiness. The progressive movements, therefore, of a person affected with paralysis agitans, were not unfrequently hurried and quick. This had not been the case with Boeri; on the contrary, increased anxiety and effort, instead of quickening his speed, would have brought him to the ground. The affection of the tongue was also greatly aggravated by efforts to speak, either faster or plainer than usual: when the mind became excited, and the endeavours to speak were increased, the power of articulation was almost lost. Thus we saw that the trembling from mercury appeared to agree in character with the tremors from excessive drinking, in being increased by muscular efforts, and in being greatly diminished, if not wholly arrested, by support; but that it differs in both these peculiarities from paralysis agitans.

During the whole time that Boeri had been in the hospital, his mental faculties had been perfect, with the exception of his liability to be confused by hurry, anxiety, or excitement.

With regard to the special circumstances which gave the predisposition to this disease, we knew nothing with certainty, beyond the fact, that some individuals were more, others less susceptible, and that the susceptibility varied in the same person at different times.

We found in the present case that the same individual worked with impunity five years, and kept in very good health; that on a second occasion he worked six years without suffering, although the first and the second periods had an interval of only two months. He next continued working with impunity fifteen years, at the termination of

which time the present attack came upon him, and continued for two years, until his death. We saw also, in this case, the efficacy of abandoning for a time the occupation, or, in other words, the withdrawal from the exciting cause: in each of the former attacks a two-months' absence from the source of mischief was followed by recovery, and in this last attack he could not absent himself from his employment, and the disease continued. His own impression was, that if he could have given up work he should have got well. Some men who followed this trade worked only on alternate days, and others at longer intervals, according to the susceptibility of the individual. The habits of Boeri were always regular, but the disease most frequently attacked the intemperate. In some salivation was produced, but this Boeri had always the good fortune to escape.

THE PER-CENTAGE SYSTEM.

A TRADESMAN, residing in Farringdon-street, who has given his name, has sent us a circular, containing the following statements, adding to them the subjoined comments on the disreputable partnership which they indicate:—

"*To Mr.* ———, *Farringdon-street.*

"Sir:—I beg to acquaint you, that an experienced physician may be consulted gratuitously at my establishment daily (at such and such hours), Sunday excepted.

"The physician here alluded to, Dr. Firth, Consulting Accoucheur to the Royal Westminster Lying-in Institution, author of, &c. &c. &c. &c. &c. &c., a Member of the Royal College of Surgeons, and Licentiate Apothecary of London, is eminently qualified to undertake the treatment of all diseases, whether medical or surgical.

"To those who remember the economy effected by consulting the late Dr. Walshman, it will only be necessary to say, that the like advantages may be secured by availing themselves of the present arrangement.

"Persons whose indisposition prevents them from attending at my house, and whose circumstances preclude their giving the usual fee, may be visited by the physician at their own residences, by paying a moderate charge for medicine only. I am, Sir, &c.

(Signed by a Druggist in Fleet-street.)

"May, 1840.

"P.S.—It is absolutely necessary for all persons desirous of availing themselves of the above advantages, without being compelled to call a second time before they see the Doctor, to attend during the stated hours, as Dr. Firth's other engagements compel him to leave as soon after as possible."

I forward to you the above letter, as I received it, with the superscription and address, taken, I suppose, from the Directory.

To me, a plain tradesman, who does not understand the nice shadows of professional etiquette, such a contraband manner of smuggling into practice seems most disgraceful, and like the proceeding of an adventurer. Is there no way of preventing such men from entering the profession, or of punishing them afterwards? The poor chemist is not to blame; he has only one idea—that of bringing customers to his shop. How does it happen, that this eminent author—this man of great and varied experience—has been so long to fortune unknown? The second Dr. Walshman is reduced to issue his oracles from the back parlour of a Fleet-street shop!

Dr. Firth is, clearly, a proper candidate for emigration to Sydney, or some other new settlement for bettering the worldly condition of man.

MR. ROWE'S REPLY TO MR. SMITH.

To the Editor of THE LANCET.

SIR:—The particulars of the case that ruffled this gentleman so sorely, are as follow:—

On Saturday, June 13th, I received a note *per post*, requesting me to see a Mrs. L., as soon as possible, at Walworth. I obeyed the summons, and found the lady (who had been confined a month) very much reduced, in consequence of large milk abscesses having formed in her breasts, one of which had broken, and left a sufficient cavity for a mouse to build its nest in: the other I opened, and discharged a teacupful of matter, in the state usually found in such cases. Having done this, I wrote a prescription for a light tonic, ordered fomentations and poultices to be applied to the breast I had just lanced, to be followed in two or three days by a warm spirit lotion to both; and these instructions and a prescription I left for her medical attendant, of whom I knew nothing. Nor am I aware that in so doing I gave him any just cause of offence, or did anything that any other surgeon would not have done, sent for as I was. Ought he not to have prevented his patient's breasts from coming to this state, as she never intended to suckle her child; or as soon as matter, if it may be so called, did form, ought he not to have discharged it, and prevented the destruction of so much integument and pain? Any Tyro would answer—Yes!

I was again sent for by Mrs. L. on the 25th, she having discarded Mr. Smith, because the last time he saw her, he punctured her breast where there was no *matter*, which astonished me, for it was full, and I dis-

charged five or six ounces at least; but on expressing my surprise, was informed, he was not in a state likely to find out where to make an incision.

I think the Editor might have reserved his comment till he heard my reply. I have not been thirty years a member of the College without ascertaining how to act towards my medical brethren, and how to treat a lady's breast who does not intend to suckle her infant.

MATTHIAS ROWE, M.R.C.S.

24, Woburn-place, July 5, 1840.

*** Mr. Rowe's reply is anything but satisfactory. We will grant, as he asserts, that he was sent for to the patient, that he may have found her in a state requiring surgical assistance. But in such a case, medical etiquette, even without "thirty years'" experience, demanded, that he should have ascertained if another medical gentleman were in attendance, and if so, common politeness would have dictated, that the presence of that gentleman should have been invited. That the lady was Mr. Smith's patient is not denied by Mr. Rowe; but he appears disposed to trample down the charge against him, on the strength of having learnt no better, after being thirty years a member of the College

ADVERTISEMENT (No. 2028) IN PUFFERY OF DR. A. FRANZ.

To the Editor of THE LANCET.

DR. FRANZ presents his compliments to the Editor of THE LANCET, and informs him that the paragraph-advertisement in the "Midland Counties' Herald," in which Dr. F.'s name is made to appear, was inserted entirely without Dr. F.'s knowledge; and that on inquiry it appears to have originated in the kindly intended, but secret and injudicious interference of a patient and friend, as evident from the accompanying letter.

Dr. F. trusts, after this statement, that the Editor of THE LANCET will, in justice, remove the obloquy which his hasty and unmerited censure has thrown upon the character of Dr. F.

19, Golden-square, July 6, 1840.

"My dear Doctor:—In reply to your anxious inquiry, I must acknowledge myself to be the originator of the paragraph in the "Midland County Herald," which has been so unpleasantly alluded to in THE LANCET.

"I can assure you, however, and you will scarcely doubt my word, that in inserting it, I had no other object in view than that of performing a friendly act towards yourself, which I considered due to you, and that I had not even a distant idea that I could in any manner risk your reputation by so doing.

"In conclusion, I can only express my deep regret at having ventured to interfere in matters connected with a profession so widely differing from my own commercial pursuits. Believe me, my dear Sir, ever sincerely and gratefully yours,

"J. J. ULIMCK."

"10, Addle-street, Wood-street,
July 6, 1840."

*** The name attached to the MS. of the above note is not legible, but is given here as nearly as the printers can decipher the letters. "Robson's Directory" presents the name of J. JAMES, comb-maker, at 10, Addle-street. The advertisement—after this defence—is worth a second examination.

TO CORRESPONDENTS.

The notice of *Mr. Owen's* letter last week ought not to have conveyed the impression he mentions. The opinion which it contains did not apply to Mr. Albert Owen, but was expressed on a case which Mr. Owen stated to the Editor. The question of Mr. Owen was put in general terms, and received a general reply; but as the remarks in our correspondent's letter might have received in the neighbourhood a personal application, we gave an answer to his question instead of publishing the letter itself. Since Mr. Owen did not enter into particulars, it was not right to insert an insinuation which no one, worded as it was, could have demanded a right to repel in our pages. We trust that no surgeon at Tenterden has been aggrieved by the unwarrantable intrusion of a brother practitioner amongst his patients, but we much fear that misconduct of that kind is on the increase in the profession. The letter of Mr. Owen was destroyed after the notice was written.

Mr. Nasmyth's Paper.—At p. 569, col. 1, line 35, of the present Number, after the words, "he will there find, &c.," to the end of the sentence, the following passage should be substituted:—"That Dr. Baly, strange to say, in direct opposition to the inference Mr. O. wishes his readers to draw from Part I., p. 427, 2nd edition, *has not* fallen into the error Mr. Owen has; but, on the contrary, after noticing the researches of Retzius, adds these words:—'Mr. Nasmyth is represented as stating that the intertubulary substance of the ivory is *not structureless*, but distinctly cellular!'"

W. D. W.—Medical advice is never given to correspondents in THE LANCET.

Mr. Worthington's case has not been received.

ERRATUM.—In our last Number, page 542, col. 2, line 46, for "communes" read *connues*.